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The Muddle of the Thirties[1]

By Frederic Benham

I

THE nineteen-thirties opened in the shadow of the great depression, and during the next three or four years this shadow deepened and spread. It brought heavy unemployment and idle capacity to the great industrial nations and it brought very low prices and incomes to the countries exporting agricultural products. Nearly all the penal tariffs, import quotas and prohibitions, exchange depreciations, exchange controls, and other barriers to world trade, which came into being during this period were introduced by the various countries in order to fight the evils which the depression brought in its train. We can see now that their policies were short-sighted and mistaken. The defensive measures of one country, especially of an important trading country, inevitably reacted upon others, and often compelled or provoked them to retaliate. In the end, the first country lost instead of gained, even from its own nationalist standpoint. But this was far from obvious at the time. A Government saw distress, unemployment, bankruptcy, within its borders. Something had to be done. The arguments for the measures it adopted seemed powerful and convincing. Often there appeared to be no possible alternative. Yet the result was an orgy of economic nationalism, which persisted throughout the decade. The volume of world trade remained well below the pre-depression level, and most of the exporting industries and districts of the world continued to suffer either from low incomes or from heavy unemployment or from both.

Some of the smaller countries, such as Belgium and Denmark, were more sinned against than sinning, but they alone were powerless. They could not turn the rest from their *sauve qui peut* attitude. Each of the others acted as if the depression was something coming from outside, like a blizzard or tornado, against which it had to defend itself as best it could. Yet in fact the depression was a man-made phenomenon. It could and should have been conquered by common action. Why did not the nations get together?

Rather belatedly, they did. The World Economic Conference met in London in the summer of 1933. It was completely and utterly wrecked by President Roosevelt's pronouncement that the United States was determined to follow its own independent monetary and economic policy. Readers may remember the remark of the oil-man who keeps popping up in the pages of Claire Booth's *European Spring*. People blame this and that for the muddle, he says, but it is funny that nobody thinks of blaming the person really responsible—Mr. Franklin D. Roosevelt. Nowadays we rightly regard the President as a great and wise statesman. Yet there is some force in the oil-man's observation.

The thesis of this article is that economic nationalism is a beggar-my-neighbour policy and does not pay. We shall illustrate it by discussing agrarian protection in Western Europe and the external monetary and trade policies of Great Britain and the United States. The first of these three topics must be treated at some length, although even so our account will be very compressed. It raises issues which will be of great importance after the war.

II

An outstanding feature of the great depression was the heavy fall in the prices of most agricultural commodities. Here we are concerned mainly with foodstuffs, for it was mainly against imports of foodstuffs that high protective barriers were raised. The most important foodstuff, from the standpoint of international trade, was wheat.

The British price of Canadian wheat averaged 51s. 4d. in 1929, 37s. 6d. in 1930, and 26s. 3d. in 1931. By 1932 the world prices of cereals, sugar, dairy produce, and vegetable oils were nearly all down to half, or less than half, their pre-slump levels. Let us see how this came about.

The surface explanation, for most agricultural commodities, is that stocks had been accumulating. But of course this was merely the visible symptom of the real trouble, namely, that production was outrunning demand.

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[1] This article will also appear at about the same time in an American journal.

When people become better off they spend more on goods and services which they previously regarded as luxuries or semi-luxuries. People at a "western" standard of living are unlikely to spend more on the simpler foodstuffs: they may spend less. For example, the rising trend of the general standard of living in Great Britain was accompanied by a fall in the *per capita* consumption of wheat flour: from 252 lbs. a year in 1861–65 to 212 lbs. in 1936. Until recently the fall in consumption per head was more than offset by the rapid growth of population, but this has now ceased or considerably slowed down in most western countries.

Meanwhile technical progress has been taking place in agriculture. One man's labour on the land was providing enough food, as time went on, for more and more mouths. And whereas technical progress in the manufacture of motor cars, for example, may lead to a fall in their prices and a great increase in their sales, so that more workers than before can be employed in the industry, this does not apply to most branches of agriculture. As we have seen, there is not much scope for expansion among western peoples in the *per capita* consumption of the staple foodstuffs. Hence, broadly speaking, the result of progress in the art of agriculture is that fewer workers are needed on the land. The decline of the agricultural population is a symptom of material progress; from the purely economic standpoint, it is a matter for congratulation. Fewer workers are needed to grow food; more are available for producing other things, thereby raising the standard of living. But this means, still speaking very broadly, that agriculture is a declining industry and tends to be relatively depressed. For too few farmers give up agriculture and turn to some other occupation. Agriculture is therefore overcrowded. Although the more efficient farmers do well, the general level of profits and wages is lower than in other branches of production.

During the nineteen-twenties technical progress in agriculture was very rapid. But it reduced costs more in overseas countries than in Western Europe. For example, the new and improved types of agricultural machinery could be, and were, used more widely and to better advantage in the New World, where land was plentiful relatively to labour. We may quote two illustrations of how they reduced costs in Canada. A man with four horses ploughed on the average three acres a day at a cost of $2.33 an acre; a man with a three-plough tractor covered 6.7 acres a day at a cost of $1.52 an acre. In the spring-wheat territories of Western Canada "1925 was the first year in which combines of the modern type made their appearance in any considerable numbers. The costs, estimated on the basis of one machine harvesting 600 acres, each acre yielding twenty bushels, were 9½ cents per bushel for the whole operation, as against 17½ cents per bushel by the old method of binder followed by thresher".[1] In Western Europe the larger number of relatively small farms into which the land was carved up, and the much lower levels of wages, made mechanisation more difficult and less cost-reducing. The same applies, on the whole, to other aspects of technical progress. Thus the discovery of more prolific types of sugar-cane benefited tropical producers as against European and other growers of sugar-beet, and improvements in dry-soil farming could be applied mainly in Canada.

We can now see why agricultural prices were sagging toward the close of the twenties and why European producers of most foodstuffs were finding overseas competition more difficult to meet.

When the prices of foodstuffs began to tumble, most European countries protected their farmers by placing severe restrictions on imports. The protecting countries most important as markets were Germany, Italy, and France, but most of Western Europe followed the same course: Belgium and Denmark were notable exceptions. Agrarian protection had been increasing before the great depression. In 1924 64 per cent. of the world imports of wheat entered free; in 1929 only 39 per cent. Mussolini began his "battle of the grain", to make Italy more self-sufficient in cereals, in 1925. Both Germany and France substantially raised their import duties on meat around 1927 and 1928. But all this was quite dwarfed by the formidable barriers erected during and after 1930. We need not give details of the various weapons—high tariffs, import quotas, milling regulations, exchange control and so forth—which each country employed. The results speak for themselves. By 1933 imports of wheat into the principal European importing countries (excepting the United Kingdom) had dropped to half their 1925–28 level; imports of beef and veal and cattle had fallen to one-third. Germany, the great continental importer of butter, was taking only some 60,000 tons as against 100,000 before the slump.

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[1] *The Agricultural Crisis*, Vol. II, pp. 14–15. League of Nations. 1931.

The " protectionist " countries maintained severe restrictions on the imports of most foodstuffs throughout the thirties, even after world prices had made some recovery. The most important commodity affected was wheat. Most European countries approached self-sufficiency in wheat. Indeed, France at times, when her harvests were good, *exported* substantial quantities. The world price, which she received for her exports, was less than half her internal price; the French government made up the difference.

A full discussion of this policy of agrarian protection would need far more space than we can spare. We have made broad generalisations. In fact, relevant circumstances varied between countries and between commodities. Opinions may reasonably differ about what would have happened if some other policy had been adopted. And two persons who agree upon what happened, and upon what would have happened under different policies, may yet pass quite opposite verdicts because they disagree as to which *ends* are more desirable.

If the sole aim is to make the general standard of living as high as possible, then agrarian protection as a long-term policy is stupid. It prevents products from being grown where they can be grown with less effort and therefore more cheaply. It compels people in the protected countries to pay more for their food in order to keep more persons than are really needed at work on the land. It may prevent farmers from switching over to those products, such as fruit and vegetables and dairy produce, for which the demand is expanding.

All this happened in the protected countries of Europe. They grew more wheat; and the New World, where wheat could be produced far more cheaply, was compelled to grow less. They grew sugar-beet and kept out the much cheaper cane sugar of the tropics. For these and other commodities technical progress had turned the balance of advantage in favour of overseas countries; Europe refused to accept its benefits. The result was that the prices of many commodities—such as cereals, sugar, meat, and butter—were much higher in the protected countries than in the world market. This left their consumers less to spend on other things. Thus the growing demand for such things as fruit, vegetables, poultry, milk, and eggs was checked; this, together with the artificial stimulus given to the production of wheat, sugar, and other highly protected commodities, held back the changes in output that would otherwise have taken place.

There is more to be said for agrarian protection as a temporary measure to meet the very serious emergency brought about by the collapse of agricultural prices. It can be urged that most farmers could not compete with imports at depression prices, that many of them would have been ruined and deprived of any occupation, their land being abandoned to weeds, if the State had not protected them. Hence, it can be claimed, agrarian protection was a kind of " public works " measure, designed to maintain employment and incomes. This argument, however, is often overstated. Agrarian Protection itself greatly intensified and prolonged the fall in prices. Many farmers, working on their own, would have carried on despite a big cut in their incomes—overseas farmers did so—although some might have switched over to products that could compete better with imports. After all, Belgian agriculture survived fairly well with little State assistance. Governments could have relieved serious hardship by compassionate grants to individuals and might have tackled the unemployment problem in other ways.

The preceding paragraphs assumed that the sole aim should be to raise the general standard of living. Many people, however, refuse to accept that assumption. They believe that the nation's agriculture should be preserved even at some sacrifice of material prosperity. Nobody can prove them wrong, for an expression of opinion such as this (a " value-judgment ") is by its nature incapable of being proved or disproved. But we may ask some questions.

Is this view based partly on the belief that countryfolk are healthier than townsfolk ? If so, it rests on very weak foundations, for statistical evidence gives little support to this belief. The same applies to the belief that people are happier in the country; in fact, many farm workers long to escape to the comforts and amenities of a town.

What exactly is desired ? Is it a large population living in the country ? The best way to get this is to stimulate, by providing such facilities as cheap electricity and good roads, the movement of industries into rural surroundings. For the numbers in agriculture must decline. Protection in Europe has slowed down the decline but has not prevented it; German agriculture, for example, lost over a million workers between 1933 and 1939.

If what is desired is not merely a populous countryside but also a flourishing agriculture we may still ask why the farmers of Western Europe could not be encouraged to switch over more to commodities for which the demand is likely to expand. For these are the very commodities in which nearness to the market gives European farmers the most shelter from overseas competition. Are cows inherently less desirable than wheat? If not, would not government subsidies enable poorer people and children to consume more milk and be a better way of helping agriculture than high protection on wheat?

But asking questions like these is shadow-boxing in the dark. We must accept the fact that if somebody declares that he favours agrarian protection and supports his opinion only by vague phrases—such as " the peasant proprietor is the backbone of the nation and must be preserved ", "*le blé, c'est une chose sacrée pour nous français* ", " the mystical joys of working on the soil in close communion with nature ", " the land must not die "—we have no effective answer. We can only make sure that he fully realises what his policy costs—how much it keeps down the general standard of living.

And now at last we can show how agrarian protection affords such a striking illustration of our thesis. The countries which adopted it may have thought that the cost was worth while, although a cynic would suggest that the great political influence of their numerous farmers had something to do with their decision. But they took the decision after balancing the advantages and disadvantages to themselves alone. Had there been a world planning authority, acting in the best interests of the world as a whole, it would certainly have rejected this course. For the loss and suffering thereby imposed upon the overseas and Danubian countries, which played no part in the calculations of the protectionist countries, far outweighed the dubious " gain " to the latter.

The former, the agricultural exporting countries, were very hard hit by agrarian protection. The prices of their exports were forced down, by this contraction of their markets, to much lower levels. Most of their farmers, having no alternatives to which they could readily turn, perforce went on producing. But their incomes were reduced to half or less than half what they were before the depression; they found it difficult or impossible to pay their debts; the fall in their expenditure led to unemployment and idle capacity in the industries of their own countries; and there was widespread distress and dislocation.

Before the depression most agricultural-exporting countries—except, of course, the United States—had been increasing their imports, for they had been borrowing abroad. Out of a total of nearly 10 milliards of dollars of new foreign securities issued during 1924–28 on the four chief capital markets of the world (the United States, the United Kingdom, the Netherlands, and Switzerland) over half was issued on behalf of agricultural countries outside Europe. This flow of capital ceased in 1929. These countries were then confronted with the problem of creating a surplus of exports over imports, out of which they could pay interest on their foreign debts. But it was impossible for them to maintain, let alone increase, the value of their exports. On the contrary, this was greatly reduced by the fall in agricultural prices. The only possible solution was a most drastic curtailment of their imports. In any event, they would have spent much less on their imports, for their incomes had fallen and the flow of loan-money from abroad had ceased. But on top of this they checked imports by letting their exchange-rates fall and by imposing very high tariffs and import quotas and prohibitions. To what extent they were absolutely compelled to do this is perhaps a matter for debate. But who can blame them for trying to protect and even expand their own industries when faced with shrinking markets for their exports and with unemployment and distress at home? They, too, could practice the art—not so noble—of economic self-defence.

Their drastic curtailment of their imports was a direct blow to the industrial countries, for their imports were mainly manufactures. Between 1928 and 1932 the gold value of exports from industrial countries to agricultural countries outside Europe declined by no less than 70 per cent. The European countries which had adopted agrarian protection naturally suffered with the rest.

We can now see the full consequences of high agrarian protection. It did protect agriculture, notably the production of cereals and sugar and meat, in the countries adopting it. It did help their farmers over a difficult time. On the other hand, it held down the standard of living in these countries and prevented or checked them from adapting their economic structure, and in particular their agriculture, to world conditions which had permanently changed. It inflicted severe loss and distress on the agricultural-exporting countries overseas (and in South-East Europe). It compelled or provoked the latter to retaliate by drastically curtailing their imports, and this was a severe blow to the exporting industries of the countries which had adopted it. The curtailment of imports was accompanied by falling exchange-rates or exchange restrictions or mounting tariffs and prohibitions, which increased the risks and uncertainties of international trade. Moreover, agrarian protection not only forced down the world prices of foodstuffs to lower levels; it also delayed for years any substantial recovery from those levels. In short, it intensified and prolonged the world depression against which it was intended to protect the countries adopting it. Who can doubt that if all this had been foreseen, and if common international action could have been arranged, each country would have acted differently—for the benefit of all? In fact, it was all a hopeless and pitiful muddle.

One major point remains. " Defence," said Adam Smith, " is more than opulence." What about the argument that agrarian protection was necessary to make the countries in question more self-sufficing and therefore more prepared to face war?

One answer to this is that the best way to prepare for war is to build up strong armaments, and that more resources will be available for this purpose if full advantage is taken of cheap imports of food. Moreover, the wisest course might be to hold very large stocks of such commodities as wheat, sugar, and whale-oil, and to have a very large " herd on the hoof " which could be gradually killed off, the pastures being ploughed and used to grow cereals and potatoes and so forth, as the war went on. But we shall not expand this answer. For defence played little part among the motives for agrarian protection in Europe between 1929 and 1932, and we hope that after this war adequate means will be found to safeguard nations from aggression.

We must refer, however, to the special case of Germany. She suffered, like other countries, from the depression, and she adopted agrarian protection and exchange control. Then, in 1933, Hitler came to power. This introduces a new and sinister element into our picture, for his purpose was to prepare for war. With that object he retained exchange control, using it as an instrument of central planning; he deliberately made Germany less dependent on imports, and fostered her trade with neighbouring countries in South-East Europe. It is all very well to point out that his economic policy, whilst greatly increasing employment in Germany, kept down the German standard of living. It is true that Buna rubber and oil-from-coal were very expensive to produce. It is true that a thousand workers in the British export industries procure as much food, imported in exchange for their products, as two thousand German agricultural workers produce directly from their soil. But those who stress such facts remind me of the man who grumbled because a rival pair had bid and made seven no-trumps in a bridge tournament, although the woman with the lead had an ace. " She did not double," he complained, " and she did not lead her ace." " You are wrong to indict her on two counts," he was told. " If she had made up her mind not to lead her ace, she was quite right not to double." In the same way, once Hitler had decided to prepare for war, knowing that he would be blockaded, he was right to follow the economic policy which he did. It was the decision to prepare to wage war which constituted his monstrous crime—and blunder.

We may point out, however, that the deadly nightshade appears among the flowers of folly only if it finds a suitable climate and soil. The economic nationalism of other countries, the Ottawa system, the closing door in the colonies, lent colour to the propaganda that Germany was economically encircled. It is now plain that the failure of the World Economic Conference destroyed the last chance—if it *was* a chance—of peaceful and friendly co-operation between Germany and the rest of the world.

III

We turn to Great Britain. During the great depression, she took two most spectacular steps by abandoning her long-established policies of adhering to the gold standard and (on the whole) to free trade. She went off gold and set up an Exchange Equalisation Account. She adopted a general protective tariff, supplemented by import quotas, and this enabled her to set up the Ottawa system of imperial preference and to bring pressure on other countries to concede advantages for British exports.

As usual, the case for economic nationalism was strong. During the twenties Great Britain was lagging behind her economic rivals. Her share in the world's exports was falling and her unemployed, mostly in the exporting industries and districts, seldom numbered less than a million. With the depression her exports fell heavily, important home industries (including iron and steel and agriculture) were in distress owing to the competition of imports at slump prices, and unemployment rose towards three million. In the realm of theory, Mr. Keynes and others had declared that the troubles of her exporters were due largely to the over-valuation of the pound sterling and had exposed the naïve belief that every import automatically results in a corresponding export. Other countries were increasing their barriers against imports, including those from Great Britain and her Empire. The Dominions had long given preference to goods coming from Great Britain and were demanding preferences from her: she could give them only by taxing similar goods coming from non-Empire countries. Thus there were several strong arguments for a change of policy.

Great Britain went off gold in September, 1931. Whether she was compelled willy-nilly to take this step or whether different and stronger measures would have enabled her to stay on is a matter for debate. The great majority of experts believe that anyway she was wise to go off. A powerful but rather technical argument in favour of their view is the following: Her exports had fallen, therefore her imports had to be cut down. To cut them down by deflation, implying bankruptcies and increased unemployment, would probably have required a relatively large fall in her total money income, for her imports were mainly more or less essential foodstuffs and raw materials.

My own view is that she both could and should have stayed on, but as I am in a very small minority I shall say nothing in support of it. I do urge, however, that she should have quickly returned to gold at a somewhat lower parity. This action might have stabilised exchange-rates; had the devaluation of the pound been only a moderate one, it might have prevented future devaluations of other currencies, notably the dollar and the franc.

In fact, the opinion rapidly gained ground in Great Britain that her abandonment of the gold standard—which a National Government had been elected to prevent—was a blessing. She had shaken off her golden fetters. She could now permit the exchange-value of the pound to fall, thereby promoting British exports and protecting home industry against imports, to whatever extent seemed desirable.

British wages and other costs did not rise to neutralise this " competitive advantage " to British exporters. Moreover, most of the British Empire and a number of other countries remained linked to sterling at the old level. Hence the fall in the pound was a heavy blow to the countries remaining on gold. Goods produced in the " sterling area " were offered for sale, in all markets, at much lower gold prices than before. This imposed severe deflation on the " gold " countries ; their prices and incomes had to fall to meet this competition. Moreover, the claims of their industries for higher protection were greatly strengthened.

The British Exchange Equalisation Account was set up to smooth out temporary fluctuations in the exchange-value of sterling. As time went on, very large movements of " refugee capital " took place. Much of this came to Great Britain. Its owners feared that their own currencies (for example, francs) would fall in value; they thought that sterling, which had already fallen, was comparatively safe. Had nothing been done, these inflowing funds would have pushed up sterling. Hence the Account absorbed them, giving sterling in exchange for foreign currencies, which it turned into gold. (By the spring of 1938 it had bought over 100 million ounces of gold.) Unfortunately this gave the impression that the Account was deliberately keeping down sterling, in order to help British exports. This impression played some part in inducing other countries to retaliate by protective measures and by promoting their own exports through devaluation or subsidies.

The British tariff was on the whole quite moderate. But Great Britain was a very important market; she bought about a sixth of all goods entering world trade. Hence her tariff, and her quotas, did inflict considerable hardship on foreign countries. We may mention particularly Denmark, from whom she imported only about half as much bacon as before; the Argentine, who was the chief sufferer from her quantitative restrictions on meat imports; and the countries in the European steel cartel, Belgium, Luxembourg, France, and Germany, whose exports to Great Britain (mainly of semi-finished steel) were drastically cut down.

Under the Ottawa Agreements the proportion of British imports coming from the Empire increased from 29 per cent. in 1930 to 40 per cent. in 1938. Foreign resentment against imperial preference was increased by the attitude of Great Britain towards proposed low-tariff agreements between neighbouring countries, such as Holland and Belgium or the Danubian States. She insisted on her most-favoured nations rights and thereby scotched all such projects. Her Dominions wanted adequate protection for their own manufacturing industries, so that on the whole they gave increased preference to Great Britain mainly by raising their duties on foreign goods above pre-slump levels. Moreover, the Ottawa Agreements virtually prevented Great Britain and most of her Dominions from reducing their tariffs to foreign countries on important classes of commodities.

Great Britain tried to promote her exports by trade treaties. She did obtain special privileges from countries to whom the British market was of vital importance. This added to the difficulties of her rivals in trying to sell to these countries.

Nobody will deny that British monetary and commercial policies inflicted losses on other countries. But did they help Great Britain?

The predominant view is that they did. Certainly Great Britain made a striking economic recovery. By 1937 output, employment, profits, and real wages were all substantially higher than in 1929, the peak pre-slump year.

Yet *post hoc* is not always *propter hoc*. If it were, then the British recovery should be attributed partly to balancing the budget and cutting down on public works. I do not think that most opponents of the gold standard and free trade would welcome that conclusion. Let us therefore try briefly to trace causes and effects.

The two outstanding features of the recovery were the building boom and, later, re-armament. Neither obtained much help either from protection or from the depreciated pound. Indeed, perhaps the main reason for the former was the low price of imported food, which increased the spending-power available for housing. Great Britain benefited from the misfortunes of overseas agricultural countries and—unlike France and Germany and Italy—she did not deny herself this benefit by excluding their products, although this was mainly because her Dominions could supply most of her needs and she did not choose to tax or greatly to restrict her imports from them.

Employment in protected industries was doubtless greater than it would have been under free trade. But it did not expand. On the contrary, the only inquiry made into this matter (by the Import Duties Advisory Committee) showed that between 1930 and 1934 employment in protected industries fell by about 50,000. Over the same period, employment in other industries increased by some 400,000. These figures do not include agriculture. This received some protection (although not against the Empire) and other assistance, yet the numbers engaged continued to fall. It is true that some investment was stimulated in protected industries, notably in iron and steel, but it is difficult to show any considerable expansion in employment or activity due to increased protection from foreign competition. Her re-armament programme required greatly increased imports of iron and steel.

What about British exports? Advantages for them were obtained in the Empire and in certain foreign markets, notably in Scandinavia and the Argentine. Yet the brute fact remains that throughout the thirties the volume of British exports was 25 to 30 per cent. below the 1929 level (except in 1937, when it was only 20 per cent. below). Unemployment was greater than in the twenties, and most of it was in the exporting districts. The tariff and the depreciation of the pound had *not*, as was promised, cured the troubles of British export industries—on the contrary. The British recovery was a *home* recovery. It took place in spite of her economic nationalism rather than because of it. The "competitive advantage" obtained for British exports, of the "competitive disadvantage" removed, by going off gold came to nothing, or less than nothing. Other countries retaliated by protective measures, and as time went on they devalued or depreciated their own currencies so that within a few years the pound sterling stood higher than ever in relation to most currencies. Great Britain had merely played her part in creating a period of uncertainty and instability and restrictionism in the economic world.

IV

What about the United States? She stopped lending abroad; she raised her tariffs; and she devalued the dollar. These actions were not exactly contributions towards international peace and prosperity. Yet there seemed very strong grounds for them.

We must remember that the depression swept upon the United States with terrific violence. She was hit far worse than most countries. All sections of the community suffered. Security prices collapsed, there were well over ten million unemployed, many of her farmers were ruined and could not pay their debts, and—largely for this reason—a number of local banks were in serious difficulties. There seemed every excuse for desperate measures. This applies particularly to the devaluation of the dollar, which on the face of it was a flagrant and unnecessary piece of economic nationalism. But let us take our points in order.

During the years 1925 to 1929 a number of countries, mainly in Central Europe and South America, had been borrowing on a large scale from the United States. This had raised their money incomes and expenditures, but a considerable part of the loans had been used in ways which did little to increase output. American investors had been incredibly careless about what became of their money. By 1930 it was dawning upon them that much of it would never be repaid. Naturally, they decided they had had enough; we cannot blame them. But the borrowing countries were rather suddenly placed in a worse position when the stream of American money became a trickle or dried up completely. The moral is not that American investors should not lend abroad—far from it. The moral is that they, or the government, should take care that their money is used honestly and wisely.

The notorious Hawley-Smoot tariff was imposed in the middle of 1930. Duties on many items were substantially raised; some became almost prohibitive. Thus the second largest market in the world was made much more difficult to enter. We have pointed out that very heavy unemployment and great distress prevailed in the United States. But to other countries, also facing economic adversity, this was more like a blow in the face than a helping hand.

Moreover, it may well be asked in what way the United States expected her debtor countries to pay their interest. She did not want a lot of foreign banknotes. They were not gold-producers. They could pay only in goods. Obviously they could send her goods, from the dollar-proceeds of which they could meet their obligations, only if she was prepared to have a surplus of imports over exports. But this was exactly what she did not want. She raised her tariff for the very purpose of keeping down her imports. Surely it was illogical of her to complain that her debtors were defaulting !

We turn to the devaluation of the dollar. When President Roosevelt came to power in March, 1933, he decided not to keep the dollar tied to gold, as it had been, but to let it fluctuate. In July he told the World Monetary and Economic Conference that the United States felt obliged to follow an independent monetary policy. In January, 1934, the dollar was formally devalued, being linked to gold again at some 60 per cent. of the old parity. There was no certainty, however, that this was final; it could be devalued still further if the President later thought this desirable.

The dollar could have been kept on gold at the old parity. It is true that for a time the United States tended to lose gold, but this was largely because people expected that the value of the dollar would be deliberately reduced. Her stocks of gold were very large and could easily have met all demands upon them had the Administration determined to stay on the gold standard without devaluing. In fact, during the latter part of 1933 the Administration did its best to keep the dollar down by buying gold and silver from abroad. The new parity—of 35 dollars an ounce—was far below equilibrium level. This is obvious from the very large inflow of gold which has taken place since, mainly because America bought so much less from other countries than she sold to them.

The fact is that the dollar was devalued because the President hoped that this would raise prices within America and because he apparently believed that higher prices, however brought about, would promote recovery. This is the key to much of his policy at that time; he was badly advised. Let us repeat, however, that he had every excuse for trying desperate measures. The depression had been getting worse, in the United States, as time went on. Over five thousand banks had failed during 1930–32, and in March, 1933, the President had to proclaim that all banks would close their doors for a week. The number of unemployed had risen to some fourteen million. Almost any new experiment seemed worth trying. And although the proceedings of the World Conference fizzled out after the cold water of the President's announcement, they might not have achieved much anyway. If other countries, notably France, had been willing to carry out a co-ordinated policy of monetary expansion, the President might have co-operated. In short, it is not easy to apportion blame. All we can say is that the depreciation of the dollar destroyed any chance of a general stabilisation of exchange-rates at that time and made it more difficult for other countries to sell to the United States or to compete with her in common markets.

Its effects on the economy of the United States are difficult to disentangle from those of all the other important measures which formed part of the New Deal. We can safely say, however, that the President's hopes were disappointed. The United States made no real recovery. Unemployment remained around ten million; at the census of November, 1937, was nearly twelve million. The volume of American exports remained far below pre-slump levels. The country acquired a great deal more gold, which it locked away; we can see now that it would have done better to buy and store metals and other materials needed for re-armament. Although the United States exports only ten to fifteen per cent. of its output, it is such a wealthy country that the absolute amount of its foreign trade is large, and so its external monetary and trade policies are very important to the rest of the world. And by now it is surely clear that the peace and prosperity of the rest of the world are very important to the United States. Economic nationalism never pays in the end.

V

We have now given our three illustrations of this theme. We shall give no more. But a few words may be said here about Japan. Nothing can excuse her brutal aggression on China or her dastardly attack on Pearl Harbour. Yet we should realise her difficulties. Her great export was raw silk, going mainly to the United States. With the slump, this market collapsed. Japan turned more to a variety of other exports, including cotton and rayon goods. New barriers were raised, in the British Empire and elsewhere, against these. She had been forced, in the nineteenth century, to give up her isolation; she had copied successfully the industrial methods of the West; she had a large and growing population; now, with the depression, the West was denying her outlets for her goods and profitable employment for her people. No wonder she felt bitter and desperate!

We may conclude with a reminder that in the latter half of the thirties efforts were made to restore some stability and co-operation in the economic world. Some countries, notably the United States, were realising that economic nationalism was bad for all. In October, 1936, after the first devaluation of the franc, the United States and Great Britain and France made a tripartite agreement to keep the rates of exchange between their currencies fairly stable. The United States made a number of trade agreements, reducing its tariff. The most important of these was that of November, 1938, with Great Britain. This was also an important move away from the Ottawa system, for it involved a reduction, to which the Dominions and India agreed, on certain British preferences to the Empire. But all this came too late. Hitler was bent on aggression. The hour for international economic co-operation had passed. The thirties ended in the smoke of war.

An Analysis of Speculative Choice

By G. L. S. Shackle

A MAN who feels sure that he knows the time-rates at which the prices of all kinds of assets are going to change during some short interval measured forward from his viewpoint[1] will decide, if all assets can at his viewpoint be exchanged at given prices, to hold the whole value of his existing set of assets through this interval in the form of that *one* asset whose money price is going to increase most. But if instead of looking on a unique time-rate of price change for each kind of asset as *certain*, he entertains several hypotheses as to this time-rate, we can no longer speak of him simply as expecting one price to increase faster than any other: the degree of belief he accords to each hypothesis has now to be also considered. On what principle will he then make his choice of assets, and what actually-observed market phenomena can be accounted for by supposing that all or many individuals make use of this principle? This problem seemed to invite the application of a group of associated new concepts which I had originally devised and used in previous articles to study some problems of investment in durable equipment. These concepts are fully described, under the names of potential surprise, and focus gain and focus loss, in my "Expectational Dynamics of the Individual" (ECONOMICA, May, 1943), to which for reasons of space I must ask the reader to refer. The concept of focus outcomes makes possible a new variant of the indifference curve technique. Our main result from the use of this technique is an explanation of the sensitiveness of speculative markets to events small in themselves, minute straws in the wind which seem to have disproportionate effects. Finally our justification in substituting the concept of potential surprise for that of numerical probability, in such problems as the present, is sought in an actual market phenomenon of the highest interest both topical and permanent, namely, the nature of 'floating value' of land mentioned in the Uthwatt Report.[2]

We suppose our enterpriser to possess at date $[i-1]$ a given collection of assets, and to be faced with a set of *given* prices at which at that date he can exchange any quantities of these assets for others. We suppose him to look forward to a date $[i]$ which is so near to date $[i-1]$ that price-changes over the interval as a whole can be regarded as linear functions of time, so that he need not concern himself with changes occurring in sub-intervals within this interval. The interval thus defined, beginning at a named date $[i-1]$ and ending at a named date $[i]$, we shall refer to throughout as interval $[i]$. The date $[i-1]$ can be thought of as his viewpoint or as a date still in the future. Prices and other variable quantities will be assigned to particular dates by writing $[i-1]$ and $[i]$ as subscripts to the symbols concerned. Let $B, C, \cdots, M$ be the entire list of different kinds of assets or goods with which the enterpriser concerns himself, and let $\alpha_B, \alpha_C, \cdots, \alpha_M$ be the ratios in which, according to some hypothesis in his mind, the money prices of these goods will respectively change in interval $[i]$. That is to say, if $x_{B,[i-1]}$ is the price of good B at date $[i-1]$, and $x_{B,[i]}$ is the price it will have at date $[i]$, then $\alpha_B = \frac{x_{B,[i]}}{x_{B,[i-1]}}$. Then if each of the ratios $\alpha_B, \alpha_C, \cdots, \alpha_M$ has in his mind a unique value which he looks on as *certain*, he will decide to exchange on date $[i-1]$ all the assets he then possesses for goods of that *one* kind, say B, whose ratio α_B is highest: where there is no subjective uncertainty, his preference will be for a *single* good. (If the ratios of expected price change are equal for two or more goods, he will be indifferent between these goods or any combinations of them.)

In reality however the ratios $\alpha_B, \alpha_C, \cdots, \alpha_M$ in which the prices of the different goods will change in any interval [*i*] are not known for certain to anyone. For each such ratio the enterpriser will have in mind not a unique value but a function associating with each of a number of hypotheses concerning this ratio a particular degree p of potential surprise. Suppose that for two goods B and C the respective potential surprise functions are $p_B = p_B(\alpha_B)$ and $p_C = p_C(\alpha_C)$, and let us also assume for the purpose of our later argument, that these goods are not subject to any natural physical change. The condition $p_B = p_C > O$, for the increasing branch of each curve, will determine a one–one correspondence between values of α_B and values of α_C, each specific pair of values being associated with a specific degree of potential surprise ; and similarly for the decreasing branches. We shall call such a pair of values a pair of linked values. For any specific set of quantities of the two goods, any particular pair of linked values α_B and α_C will correspond to some specific amount of gain to be had from holding this set of quantities through interval [*i*]. There will be one particular pair of linked values for which the corresponding gain will be the focus gain from holding the given set of quantities through interval [*i*] ; and there will be another linked pair to which there will correspond the focus loss.[1] Now the forms of the potential surprise functions may be such that in every pair of linked ratios the same member, say α_B, is greater than the other. In this case the focus gain will be greatest, and the focus loss smallest, when the set of quantities held consists exclusively of good *B*, with none of good *C*. But more often one of the ratios, say α_B, will be greater than the other when both are greater than unity, but less than the other when both are less than unity.[2] In this case, by holding less of good *B* and more of good *C*, the enterpriser can have a smaller focus loss at the cost of having also a smaller focus gain. Measuring focus losses along the horizontal and focus gains along the vertical axis of Figure 3, we can display the range of choice open to the enterpriser as a curve *QQ* such that the two focus outcomes increase together. If one of the two goods in question is money, this curve will start from the origin, otherwise it will in general start some distance from the origin. For by gain and loss we mean gain and loss of money value, and by holding all his capital in the form of money, the enterpriser can make sure of having neither gain nor loss, and the situation in which he has this assurance is represented by the origin. The curve, which we shall call the gambler opportunity curve, will approximate closely to a straight line. Indeed, if instead of the focus outcomes we plotted against each other the upper and lower extremes of the inner range (that is, those amounts of gain and loss which, for the various mutually equivalent combinations[3] of goods, are associated with potential surprise barely exceeding zero) we should get a straight line. For suppose a unit of *C* exchanges, at the prices prevailing at the threshold of interval [*i*], for *U* units of *B*, that before re-arranging his holdings the enterpriser has *B* units of *B* and *C* units of *C*, and that he considers the effect of giving up r units of *C* in exchange for some units of *B*. And let us take as our money unit the value, at the beginning of interval [*i*], of a unit of *B*. Then for given (algebraic) values of α_B and α_C the total worth of the enterpriser's stocks of goods at the end of interval [*i*] will be

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[1] By the individual's viewpoint I mean the point of time at which the thoughts and decisions, which we are studying, are occurring in his mind.

[2] Expert Committee on Compensation and Betterment, Final Report (Cmd. 6386), paragraphs 23, 24.

$$\begin{aligned} Z_{[i]} &= \alpha_B(B + rU) + \alpha_C U(C - r) \\ &= rU(\alpha_B - \alpha_C) + \alpha_B B + \alpha_C UC \end{aligned}$$

while the worth at the beginning of interval [*i*] is

$$Z_{[i-1]} = B + UC$$

Thus his gain or loss is

$$Z_{[i]} - Z_{[i-1]} = rU(\alpha_B - \alpha_C) + B(\alpha_B - 1) + CU(\alpha_C - 1)$$

which is linear in r. If in this expression we insert the pair of linked values of α_B and α_C which are greater than one and carry potential surprise barely exceeding zero, we have for each value of *r* the upper extreme of the inner range. Thus the latter is a linear function of *r*, and so in a similar way is the lower extreme. Since all the quantities other than *r* which enter into the expressions for the upper and lower extremes are constants, these extremes are linear functions of each other. The upper and lower extremes are shown as functions of *r* in Figure 1, and as functions of each other in Figure 2. However, when we turn to consider focus outcomes we have to take account of possible variation in the relative weight which the enterpriser attaches to a given increment of hypothetical gain or loss and to the associated increase in potential surprise, when the increment is added to a different level of hypothetical gain or loss. Such variation would alter the distance between the upper extreme of the inner range and the focus gain, and between the lower extreme of

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[1] Thus if g is a variable hypothesis as to the amount of gain to be had from holding a given set of quantities through interval [*i*], and $p = p_B = p_C$ is the degree of potential surprise associated with each value of g, and ϕ is the power of any associated pair of values g and p to concentrate the enterpriser's interest and attention upon themselves, the focus gain is defined as that value of g for which $\phi = \phi\,[g, p(g)]$ is a maximum ; and the focus loss is similarly defined, *mutatis mutandis*.

[2] The potential surprise function for α_C will then lie wholly ' inside ' that for α_B in the manner illustrated for three functions in Figure 6.

[3] I.e. combinations amongst which the enterpriser, faced by a given set of prices of these goods at the beginning of interval [*i*], and possessing goods of a given total value at these prices, is free to choose, when deciding what set of stocks of goods he shall hold through interval [*i*].

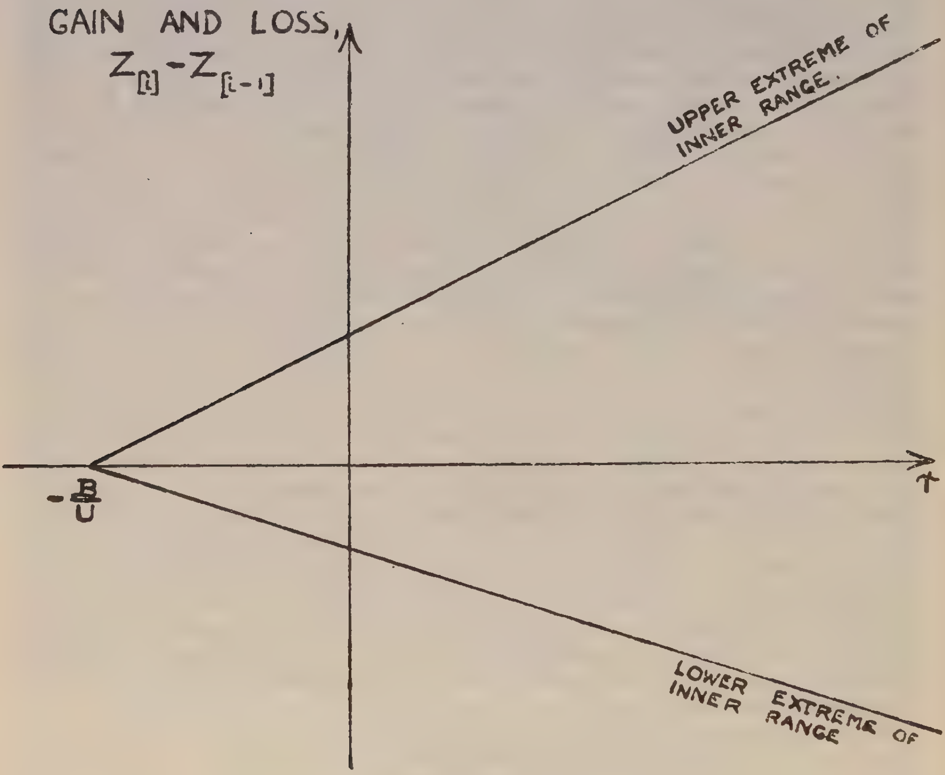

FIGURE 1

In Figure 1, those two specific pairs of linked values of a_B and a_C, which each carry potential surprise barely exceeding zero, and those two only, are considered. One of these pairs gives the upper and one the lower extreme of the inner range of $(Z_{[i]} - Z_{[i-1]})$ for each value of r. This diagram illustrates a case where one of the two goods, namely good *C*, is money. Then a_C is always unity, and if in the expression

$$Z_{[i]} - Z_{[i-1]} = rU(a_B - a_C) + B(a_B - 1) + CU(a_C - 1)$$

we put $r = -\frac{B}{U}$ we have

$$Z_{[i]} - Z_{[i-1]} = B(1 - a_B) - B(1 - a_B) = 0$$

that is to say, if the enterpriser turns all his initial stock of *B* into *C*, namely money, and holds all his capital in the form of money, his gain or loss of money value will be zero.

the inner range and the focus loss.[1] Thus where two goods only are involved the "gambler opportunity curve", as we may call the curve expressing the enterpriser's range of choice between different combinations of focus gain and focus loss, will be approximately linear but perhaps not perfectly so; and it will approximately coincide with the curve in Figure 2 which shows the upper extreme of the inner range as a function of the lower extreme. In each of these curves the relevant part is a segment of finite length bounded at one end by a point corresponding to the holding of the whole of the enterpriser's capital in the form of one good only, say B, with none of C, and at the other by a point similarly corresponding to the holding of C only with none of B; any extension of the curve beyond these points has no meaning. In the curve of Figure 2 these points are obtained by putting $r = C$ and $r = -\frac{B}{U}$ and the curve is a straight line segment joining these points.

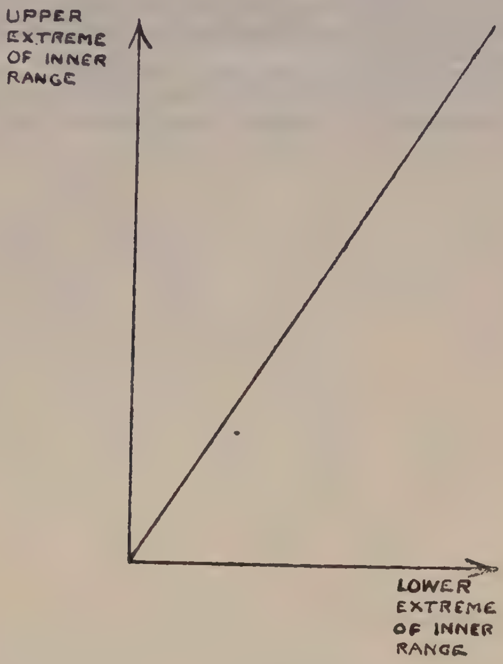

FIGURE 2

Every point with both co-ordinates positive on the plane of Figure 3 stands for a distinct pair of focus outcomes: the combination of some specific focus gain and some specific focus loss. Starting with any one such point it will be possible to find others which, to a specified person with a specified total value of stocks of goods, will be neither more nor less attractive than this point. An entire set of points mutually equal in their attractiveness to a given person at a given point of time, lying within some relevant range of focus losses (say from zero up to the total value of the particular individual's possessions) we may call a ' gambler indifference curve '. For any one person there will be a family of these curves such that any point on the plane will lie on one or other of them. Every member of this family of curves, or indifference map, will be altered in shape by any alteration in the total value of the enterpriser's possessions, for the significance to him of a given hypothetical gain or loss per unit of time depends on its ratio to his total capital (as well as to his habitual level of consumption and some other standards). Thus a gambler indifference map describes the tastes, in regard to hopes and uncertainty, of a given individual in given circumstances.

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[1] It is hard to say *a priori* whether these distances will increase or diminish as the enterpriser turns his attention to combinations of goods giving higher levels of the upper extreme of the inner range, or lower levels of the lower extreme. As a simplification which does not involve anything palpably absurd we shall assume below that the highest gain carrying nil potential surprise, and the focus gain, will change in a constant ratio to each other. Against this it might be urged that when he contemplates a low hypothetical gain the attractiveness to the enterpriser of any *given* increase in it will be stronger than when he turns to a high level of gain. In this case, if we write g for gain, p for potential surprise, and $\phi(g, p)$ for the power to focus the enterpriser's attention, $\frac{\partial\phi}{\partial g} \Big/ \frac{\partial\phi}{\partial p}$ will be numerically greater when g is low than when g is high, and if the values of $\frac{\partial p}{\partial g}$ at given distances from the upper extreme of the inner range are the same for all values of the latter, the focus gain will be *further* from the upper extreme of the inner range when the latter is low.

The gambler indifference curve which passes through the origin will indicate all those combinations of focus gain and focus loss which to the particular person concerned, in his circumstances of a particular instant, are no more or less attractive

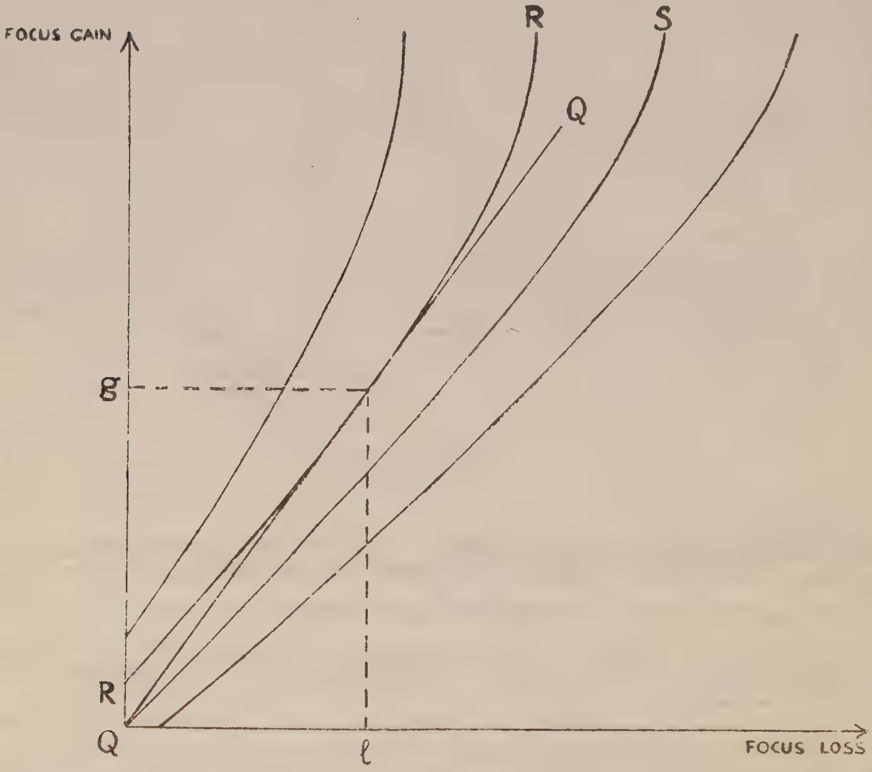

FIGURE 3

Figure 3 is drawn on the assumption that one of the two goods is money. The gambler opportunity curve *QQ* therefore starts from the origin. Amongst the gambler indifference curves which it encounters (cuts, meets, or is tangent to) the one representing the most desired situations is the curve *RR*, with which it has a point of tangency (*g*, *l*) representing a focus gain *g* and a focus loss *l*. The enterpriser will choose those determinate quantities of the two goods, *B* and *C*, which correspond to the point (*g*, *l*) on the opportunity curve. The curve *QS* is the origin indifference curve.

to him than a high degree of confidence[1] that he will experience neither gain nor loss. It seems reasonable to suppose that the gambler indifference curves will all resemble each other in the main features of their shape, and it follows that if we can infer from a general knowledge of human nature and circumstances the general shape of that particular indifference curve which passes through the origin, which we will call the 'origin indifference curve', we shall have a clue to the general shape of the indifference map as a whole. Now this origin indifference curve is likely to slope upwards to the right with increasing steepness: for a focus loss equal to the whole of one's capital would in the minds of most people need a focus gain many times as large as itself to compensate it (i.e. to render the combination equivalent to an assurance of constancy of one's capital in terms of money), while a focus loss which is a hardly noticeable proportion of one's capital can perhaps be offset by a focus gain equal to itself; and as we consider successively larger focus losses in the range between these extremes, the focus gain just sufficient to compensate each size of loss is likely to bear a larger and larger ratio to the latter. Thus in some neighbourhood of the origin we may expect the origin indifference curve to have fairly typically a constant slope of unity (half right angle) and thence to rise with increasing steepness, perhaps in some cases eventually becoming asymptotic to a finite focus loss. The typical 'gambler indifference map' will therefore resemble the one indicated by means of a few sample curves in Figure 3. In an indifference map where the

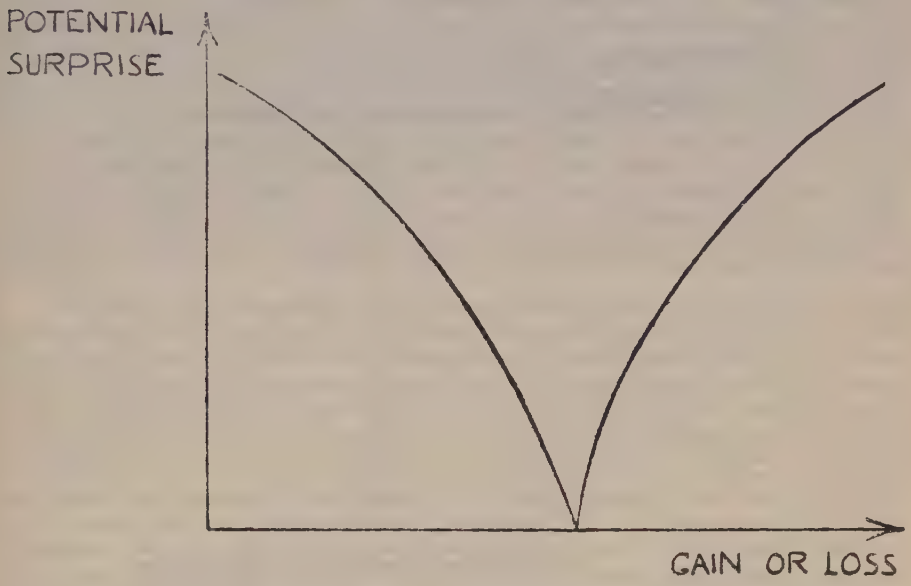

FIGURE 4

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[1] The origin in Figure 3 represents the combination of a focus gain and a focus loss both equal to zero. Now a focus outcome is by definition the amount of gain (or loss) associated with such a degree of potential surprise that if this amount of gain is slightly increased the associated increase of potential surprise will more than outweigh the extra attractiveness of the gain, so that on balance the enterpriser will be *less* interested in the new combination of hypothetical gain and associated doubt as to its possibility of realisation. In order that the focus gain may be a *zero* gain, it is necessary that the potential surprise associated with any positive gain however small should be considerable; that is to say, the amount of gain must vanish to a higher order than the associated potential surprise. But zero is not only the focus gain, it is also the focus loss, and a parallel argument applies. It follows that the potential surprise function must resemble in shape Figure 4 or Figure 5. Thus we are justified in referring to the origin in Figure 3 as representing "a high degree of confidence" that there will be neither gain nor loss: for this outcome is the *only* one carrying nil potential surprise, all others carrying some positive degree of it.

axes have the meanings here assigned to them, the combination of focus outcomes represented by any point on an indifference curve is preferred, by the individual whose map it is, to every combination represented by a point on any curve lying below and to the right of the former curve.

We have now to superpose the gambler opportunity curve on the gambler indifference map. We shall assume in what follows that the focus gain and loss always change in proportion respectively to the upper and lower extremes of the inner range.[1] Thus not only will the opportunity curve be a straight line, but when we consider the proportions in which *the inner range of* a_B, *or of* a_C, is divided by the value $a_B = 1$, or $a_C = 1$, these proportions will also apply if we consider the distances from $a_B = 1$ to those values of a_B which correspond to the focus outcomes. Thus if we assume, for example, that the inner range of a_B is *bisected* by the value unity, this will imply that, whatever the value of r, the values of a_B which correspond to the focus outcomes lie equal distances above and below $a_B = 1$. Let us indeed begin by making this latter assumption in regard to both a_B and a_C. The opportunity curve will then be a straight line bisecting the angle between the axes; and we have seen that there will be an indifference curve which in some neighbourhood of the origin also bisects this angle. If the opportunity curve extends near enough to the origin, it will coincide over some part of its length with this indifference curve, and elsewhere will lie below and to the right of it. Thus the latter will be the highest (i.e. most desirable) indifference curve which the opportunity curve encounters. If the opportunity curve and the origin indifference curve should chance to have only one point in common (which would necessarily be the extreme end of the opportunity curve nearest the origin), the enterpriser will choose that combination of quantities of B and C which corresponds to this point on the opportunity curve. In this particular case, then, his preference will be to hold his whole capital in the form of the less exciting, the more narrowly predictable in value (according to his own judgment) of the two goods. And this will still be approximately true if the two curves have a range of points, instead of a single point, in common, since this range is not likely to be large in relation to the length of the opportunity curve as a whole.

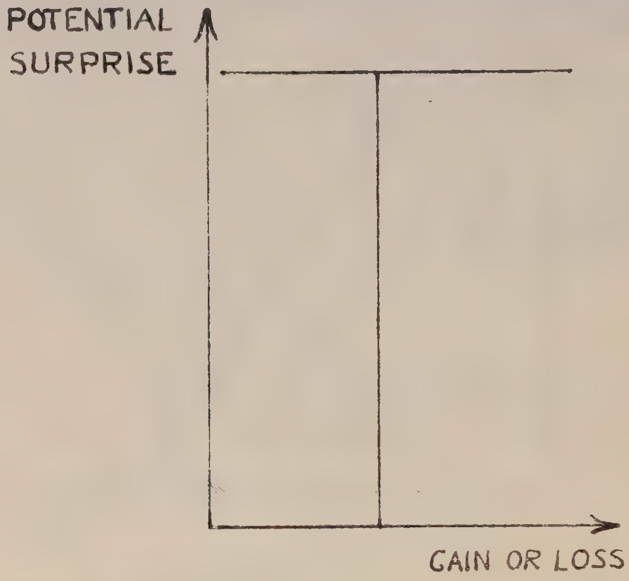

FIGURE 5

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[1] I.e. in proportion to the distances of these from the point representing zero gain and loss.

Now suppose that, instead of lying equally above and below the value unity, the inner range of α_B extends further above than below unity, while the inner range of α_C, lying wholly within the inner range of α_B, is still bisected by the value unity. In this case that end of the opportunity curve which is nearest the origin will represent two equal focus outcomes, while the other end will represent a focus gain larger than the corresponding focus loss. An opportunity curve tilted in this fashion will be common enough, since equality of the focus outcomes at one end of it will always occur if one of the two goods is money, and the other end of the curve is as likely to be nearer to one axis as to the other. The origin indifference curve will now no longer be the highest indifference curve touched by the opportunity curve: the latter, if it is long enough, will evidently be tangent at some point to one of the indifference curves lying above and to the left of the origin indifference curve, and this point of tangency will no longer be the extreme end of the opportunity curve nearest the origin, but will be some way along it, and will represent a combination of goods no longer consisting exclusively of C but comprising also some B. If the increase in the slope of the indifference curves is very gentle, so that they approximate somewhat to straight lines, a relatively small pivoting shift of the opportunity curve, in such a way as to increase its slope, will carry the point of tangency a long way up the opportunity curve to a point which implies a combination of goods consisting mainly of B. It is possible indeed, as a by no means exceptional case, that this combination will consist *exclusively* of B; for what we have called the opportunity curve is in fact a finite segment only (since the gain or loss from holding a finite quantity of any goods through a specific interval cannot be infinite) and hence although if produced far enough it would eventually have a point of tangency such as we have described, it may in fact stop short of this point, and have its extreme furthest from the origin resting on an indifference curve which it meets at an angle greater than zero.

We have in the foregoing an explanation of how some event which attracts little direct notice in its own right can initiate a boom of activity in the market, first inducing, for each of many enterprisers, a small, bodily upward shift of the inner range of, say, α_B, thus causing a slight tilting of the opportunity curve and thus a *great* change in the relative quantities of two goods (one of which may be money) which individuals wish to hold *at the initial prices*, thus inducing large offers to buy one good and causing a rise of its price, a rise which will seem to justify, or probably more than justify, the small bodily upward shift of the inner range of α_B which expressed each individual's interpretation[1] of the originating event itself, and lead, in this latter case, to a further revision of expectations and a new cycle of the same sequence of events. The validity of this explanation, it will be remembered, depends on certain assumed characteristics of the gambler indifference map which we suppose to be typical of the indifference maps of a substantial proportion of the enterprisers: these characteristics are, first, an origin indifference curve which in some neighbourhood of the origin approximately bisects the angle between the axes, and second, only a gentle curvature in the indifference curves, so that a small pivoting shift of the opportunity curve causes the point of tangency to sweep more or less from one end to the other of the opportunity curve. The shape of the indifference curves is, of course, a mathematical or graphic description of the temperament of the enterpriser concerned. The particular shape which we assume in the above argument represents what we may express in rough and ready language as a low rate of increase of the reluctance to gamble as the amount at stake, considered as a proportion of the individual's total capital, increases. Common sense approves the conclusion that, if such temperaments abound, small events will be the more readily able to generate booms of market activity.

Let us turn now to the case where more than two goods are involved. We have seen that if, say, $\alpha_B > \alpha_C$ in *every* pair of linked values, both above and below unity, the good C can be neglected. Thus when we consider more than two goods, we need only concern ourselves with cases where, say, $\alpha_B > \alpha_C > \alpha_D > \cdots > \alpha_M$ in every *linked set* in which all these ratios are greater than unity, and where $\alpha_B < \alpha_C < \alpha_D < \cdots < \alpha_M$ in every linked set in which all these ratios are less than unity. The potential surprise functions in such a case are illustrated in Figure 6. Now if, taking each of the goods $B, C, \ldots, M$ in turn, we suppose the enterpriser to

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[1] I do not mean to suggest that the upper and lower extremes of any one individual's inner range for α_B will usually move by equal amounts; still less is it suggested that the amounts of the shifts will be the same for all individuals.

hold the whole value of his capital in the form of the equivalent quantity of this good alone, we shall have on the plane of Figure 3 a number of points, each of which we may label with the name of the good concerned. Each of these points will represent a higher focus gain than any point which shows a smaller focus loss. For the value, at the beginning of interval [*i*], of the stock of goods then held by the enterpriser is the same no matter what its form. Its value at the end of the interval [*i*] will have risen (or fallen) in a ratio α_B if it is wholly in the form of good *B*, in a ratio α_C if it is a pure stock of good *C*, and so on. If, then, the potential surprise function of α_C lies wholly 'inside' that of α_B, and the function for α_D inside that of α_C, and so on, it is plain that a lower focus gain implies a smaller focus loss. Thus the points representing the paired focus outcomes attached to a pure stock of *B*, a pure stock of *C*, etc., will form a chain of straight line segments all sloping upwards to the right. But it is possible that even some of these points can be neglected: for suppose that one of them, representing, say, good *C*, lies below and to the right of a straight line segment joining two others of the points, say *B* and *D*, and within a triangle bounded by this line segment and by a straight line parallel to the loss axis and another parallel to the gain axis, as shown in Figure 7. Then it will be possible to find a point on the line joining *B* and *D* which stands for the same gain but a smaller loss than *C*, and another which stands for the same loss but a larger gain than *C*, and between these two latter points there will be a sub-segment every point of which will represent *both a larger focus gain and a smaller focus loss than C.* Plainly the enterpriser will prefer a combination of *B* and *D* corresponding to some point of this sub-segment, to the holding of a pure stock of *C*, or any combination of *B* or *D* with *C*. When all points such as *C* have been eliminated, the remainder will determine a chain of straight line segments lying *convex to the gain axis.* This chain will constitute the curve of most favourable opportunities, and the enterpriser will choose that combination of goods which corresponds to the point of tangency, or other contact, of this chain with the highest indifference curve which it meets. What combination of goods will such a point represent? Evidently it will be the combination, in determinate proportions, of that *pair* of goods which correspond to the *adjacent* '*corners*' of the opportunity chain. We are at once led to ask "Will it never seem to the enterpriser advantageous to hold a set of more than two goods?" This question opens up a theoretical issue of basic importance, We have indeed to consider whether, as we have been tacitly assuming above, the potential surprise functions for α_B, α_C, etc., are really *independent of each other when a large number of goods are considered together.*

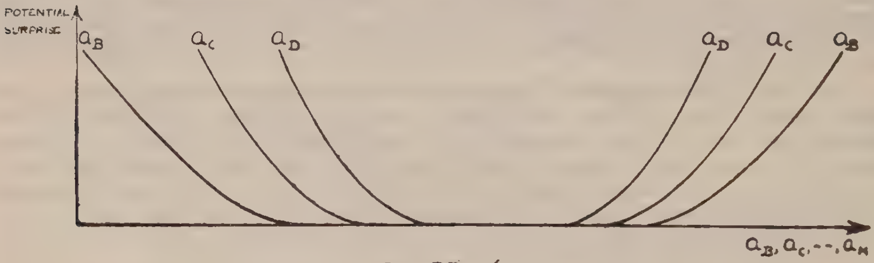

FIGURE 6

So long as the potential surprise functions for the ratios of price change remain valid in unchanging form, no matter how many goods are involved, there will always be, for any combination of more than two goods, some combination of not more than two goods which is superior (or, at least, not inferior) to it. In order to see this, let us first see how we can plot on the plane of Figure 7 a point representing the focus gain and loss from holding some specific combination of three goods, say *B*, *C* and *D*. We first plot the three points corresponding to pure holdings of these goods. Then, as we have seen, the opportunity curve for combinations of any two of them, say *B* and *D*, will be (on our assumptions concerning the relation of focus outcomes to extremes of the inner range) the straight line segment joining the two points *B* and *D* corresponding to pure holdings of these goods. Now any point on this straight line segment can be looked on as representing the focus outcomes from holding a specific quantity of a composite good made up of *B* and *D* in fixed proportions. Let us call this new composite good *E*. We can then join this fixed point, labelled *E*, to the one remaining point *C* of the three original points and thus obtain a new straight line segment each of whose points will represent the focus outcomes from holding some specific combination of *E* and *C*, that is to say, some combination of specific quantities of *B*, *D* and *C*. By a similar procedure we can determine a point representing the pair of focus outcomes corresponding to *any* combination of three goods, and analogously, for any combination of any number of goods. We shall thus get a ' swarm ' of points, which will evidently cover entirely a simply-connected region of the plane, leaving no ' holes ', or interstices, between the points within its outer boundary. But this outer boundary, above and to the left of the swarm, will be simply the gambler opportunity chain of Figure 7, and we know that any point lying below and to the right of this chain will be inferior to some point of the chain itself, that is, some point on one of the straight line segments composing the chain. Thus (except in the case where the points for three goods all lie on the same straight ' link ' of the chain) any combination of more than two goods will be inferior to some combination of not more than two goods.

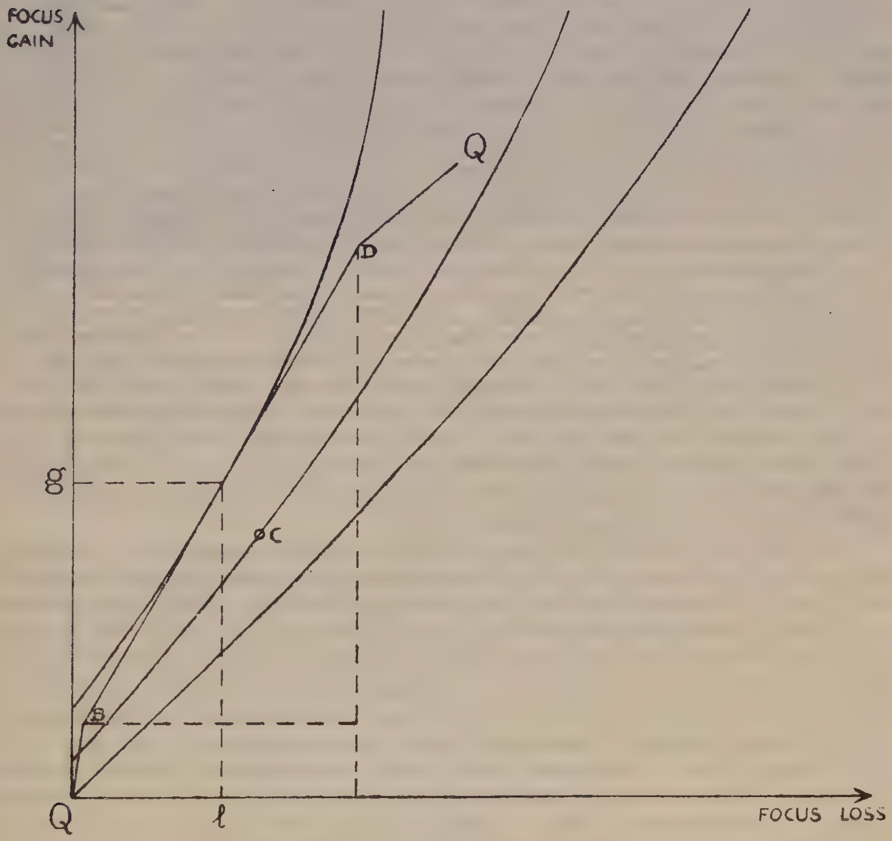

FIGURE 7

If, then, combinations of more than two goods are ever chosen, one or other of two things must in such cases be true: either some qualities of the combination besides its focus gain and loss are being taken into account; or else the assumption that the focus gain and loss of such a combination can be obtained from the potential surprise functions for the ratios of price change of the individual goods in the direct way we have described does not always hold. Let us consider this latter possibility first. It is not easy to think of any simple (mental) mechanism by which a combination of many goods could be assigned a *higher* focus gain than a pure stock, equivalent in initial value, of the most promising of the constituent goods of the combination. But there has recently come to light an exceedingly interesting phenomenon of an actual market, namely the market for land adjacent to towns, and this phenomenon, and our own immediate problem, seem to throw some light on each other. Evidence taken by the Uthwatt Committee[1] shows that, where the belt of land encircling a town is parcelled up amongst a large number of separate ownerships, the market value of each piece is such that, when the separate values are aggregated, the total is several times as great as would be warranted by any reasonable estimate of *aggregate* future building development round the town as a whole. It is as though each actual and potential owner of a plot of land near the town were convinced that, out of a far more than adequate total supply of similarly situated land, the particular plot in question was almost *certain* to be selected as part of the site for such new houses as will be required during, say, the next twenty years. We can, I think, go a long way towards explaining this curious phenomenon by supposing that, instead of assigning to his own plot the value it would have if development of it within a moderate time were *certain*, multiplied by the *probability* of this particular plot being chosen out of, say, fifty equally good plots each sufficient by itself for all likely development within a period short enough to influence current values, each owner in effect considers what I have called the focus outcomes from holding such a plot. He believes that there will certainly be some extension of the built-up area of the town: he sees no reason, other than pure accident, why his plot should *not* be the one chosen; no positive disability tending to exclude it; consequently he attaches nil or very low *potential surprise* to the hypothesis that his plot will be required; and though he must also attach nil potential surprise to the hypothesis that it will *not* be required, yet the price he assigns to the plot must be well above that which would emerge from a calculation of numerical probabilities, in order that the focus gain may not be out of all proportion to the focus loss: for if the market valuation were as low as that based on probability, the plot would offer any potential buyer a very high focus gain, even perhaps of the same order of magnitude as the difference between the value of the plot if development were *certain* and its value if non-development were certain.

Now let us suppose that the relevant belt of land surrounding a town is divided into ten equal plots each divided into ten equal sub-plots, and that the amount of land likely to be required for building development within a period short enough to influence current values appreciably is equal to one plot. Let us further assume it to be publicly known that the land required will not be taken from a number of different plots but will consist of one or other of the individual plots as a whole. And let us suppose that our enterpriser, intending to buy some of the land in the hope that the pieces he acquires, or some of them, will be selected for development, finds his capital sufficient, at the current prices, for the equivalent of one plot. He has the choice, then, of buying one whole plot or, for example, one sub-plot in each of ten plots. If he chooses this second course, his focus gain may, for some temperaments, be only *one-tenth* of what it would be if he bought the whole of one plot. This assertion will raise immediate objection from those accustomed to think only in terms of numerical probability: for they will say that, though admittedly the largest gain which the enterpriser can hope for is in the second case only one-tenth that of the first case, yet in the second case he is *certain* to make this gain, while in the first case his chances are only one in ten. This brings out, albeit in a rather exaggerated form, the essence of my contention regarding the difference between numerical probability and potential surprise. In order to enjoy with a high intensity, or even to the full, the anticipation of some gratifying outcome, a man requires only that there should be *no solid, identifiable reason to disbelieve in the possibility* of this outcome: he does *not* require solid grounds for feeling sure that it will be the true outcome. If we look upon the ten sub-plots, each from a different plot, as ten distinct commodities, it is plain that this is a case where it is by no means legitimate to derive the focus outcomes of a combination of the commodities from the potential surprise functions of price change of the individual commodities. In this particular concrete example it is the focus *gain* which is smaller for the combination of several goods than for a quantity, equal in initial value, of a single good. But there is no evident basic reason why some analogous mechanism should not operate in the case of the focus *loss*: if it did of course the combination would be preferred to any single commodity.

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[1] Expert Committee on Compensation and Betterment, Final Report (Cmd. 6386), paragraphs 23, 24.

There is not space here to consider the other class of reasons why a combination of many goods might be preferred, namely that qualities of other kinds besides its profitability as a store of value are taken into account: amongst such other qualities *liquidity* or marketability will no doubt be prominent.

AUTHOR'S NOTE :

Some months after writing this article, and just as the proofs of it have come to hand, I have seen the admirable article by Evsey D. Domar and Richard A. Musgrave on " Proportional Income Taxation and Risk-Taking " in the *Quarterly Journal of Economics* for May 1944. These authors make use, as I do, of an indifference map in which the curves have a positive slope, in contrast to the negative slope of the indifference curves used in value theory. The positive sloping curves are of course appropriate wherever one of the two variables is a *disutility* and the other a utility : and I understand that an indifference map of this type has been habitually used by Professor Hayek in his lectures to explain the individual's distribution of his time between work and leisure.

Time-Preference and Productivity: A Reconsideration

By F. A. Hayek

The question I wish here to reconsider is in the first instance the purely theoretical one of the relative importance, in determining the marginal productivity of investment, of the productivity schedule on the one hand and the so-called time-preference factor on the other. It will be remembered that on the whole the " Austrians ", that is Boehm-Bawerk and his followers like I. Fisher, F. A. Fetter, and particularly L. v. Mises, emphasised the " time preference " factor, while Wicksell and F. H. Knight tended to stress almost exclusively the productivity factor. In my *Pure Theory of Capital* I sided with the latter group, largely persuaded by Professor Knight's arguments. I believe now that in this I have been partly mistaken. Professor Knight's argument, although it seems to me still to prove its point so long as we confine ourselves to the consideration of an evenly progressing economy, does not necessarily apply to one in which the capital structure is not in full equilibrium. Though I had thought that I had made every effort to escape the " static " approach which has had such an unfortunate effect on the theory of capital, in this particular respect I seem still to have been misled by it—and that in spite of the fact that already earlier I had seen and described part of the phenomenon I am about to discuss.[1]

While the problem would at first appear to possess only theoretical interest, it seems that the answer I am about to suggest may be of some importance for clearing up certain problems raised by the historical course of the rate of interest. The fact that over a long period the rate of interest has fluctuated above a fairly constant basic level gives a certain plausibility to the contention that these fluctuations are connected solely with the monetary factors affecting it and not determined by any similar fluctuations in the underlying real phenomena—except via the changes in the money income, through which the marginal productivity of investment is adjusted to changes in the rate of interest. In particular, the rise of the rate of interest towards the end of a boom would appear, on that view, as determined solely by monetary factors, while the violent fluctuations of the volume of investment, on the other hand, appear to have been taken as *prima facie* evidence of the rapidity with which the marginal productivity of investment is reduced by any spurt of investment activity. The investment demand curve was thus conceived as being fairly steep, and the fact that the rate of return on investments never fell below a fairly constant minimum was regarded, not as showing that the productivity curve was fairly flat, but as evidence that below that minimum people were unwilling to invest; or, to put the point differently, the reason why the rate of interest did not fall below that minimum was sought not on the side of the demand for investible funds, but among the factors determining the supply of such funds. If the theoretical point I am going to make is correct, it would appear that these phenomena are capable of a different interpretation.

Before I proceed to state the correction I shall briefly restate the argument in favour of the predominant influence of productivity. The best way is to regard the problem as a special case of the general rule we must apply in deciding whether in a particular situation the relative value of two commodities depends mainly on their relative utilities or on their relative costs. " Time preference," of course, in this connection is no more than an abbreviated and rather misleading way of describing the relative strength of the demands for present and future goods respectively. I call it rather misleading because the true " time preference " of individuals is, of course, only one of the factors determining this demand position, the other being the size and distribution of individual incomes. But, for the purposes of a brief exposition of the principle, there is a certain advantage in talking in terms of a single individual and his " time preference " rather than in terms of demand on a market. It enables us to use the simple combination of a family of indifference curves and a transformation curve; and there is no difficulty in then applying the argument to a market economy.

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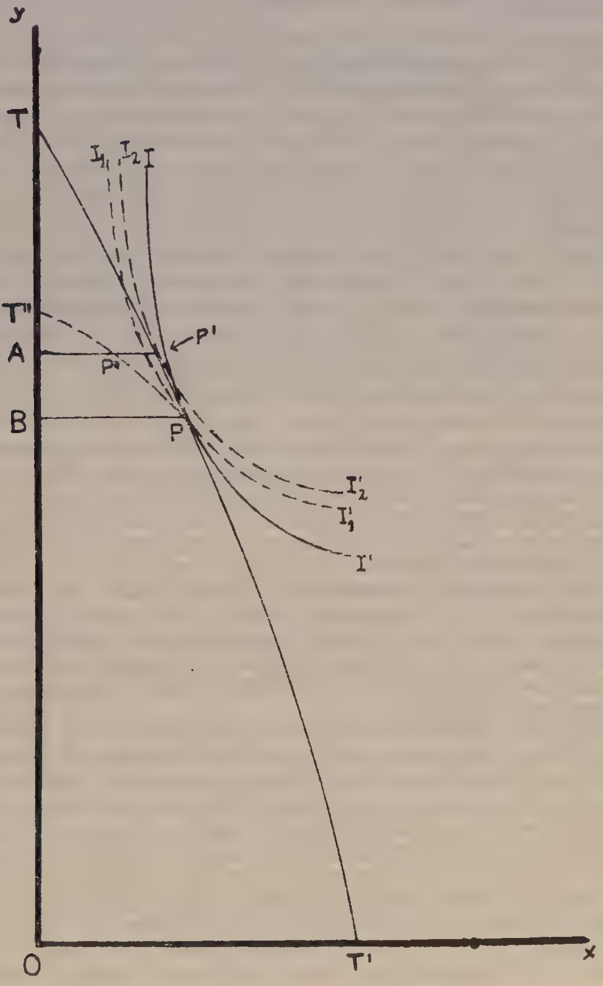
[1] See " Investment Raises the Demand for Capital," *Review of Economic Statistics*, Vol. XIX, No. 4, November, 1937, reprinted in *Profits, Interest and Investment*, 1939, pp. 73–82.

In the following diagram the transformation curve TT' represents the productivity of investment, i.e. the terms at which parts of the income of the present period (measured along the y-axis) can be converted into increments of income in all future periods (measured along the x-axis)—or the rate at which the supply of future goods can be increased at the expense of present goods. " Time preference " is shown by a family of indifference curves of which only one, the fully drawn curve II', touching the transformation curve at the point P, is shown. The argument in favour of the predominant influence of productivity on the marginal productivity of investment rests on the probability that the indifference curve will be very much more curved than the transformation curve, and that in consequence, whatever happens to " time preference ", the marginal productivity of investment is likely to adapt itself to the comparatively constant productivity.



This argument, more fully set out in *The Pure Theory of Capital*,[1] appears to me to be correct so long as we consider merely movements downwards and to the right along the transformation curve, i.e. so long as we assume a continuous and gradual accumulation of capital at a rate which at least comes up to expectations. But it does not apply to a movement in the opposite direction, i.e. to the case where the supply of capital decreases or (what for the present purposes comes to the same thing) falls short of the amount in the expectation of which previous investments have been made. In such a situation it must probably be assumed that for such a backward movement the transformation curve will have a strong "kink" at the point *P* where we find ourselves at any moment, and that in consequence, so far as such a movement backward is concerned, the transformation curve will be much more curved than the indifference curves, and "time preference" will therefore become the decisive factor.

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[1] London, 1941. Chapter XV and Appendix I.

The argument which leads to this conclusion is really the same as that which I employed in the article already referred to. In terms of the above diagram it can be restated as follows: The productivity of investment shown by the slope of the transformation curve at the point *P*, at which we find ourselves at any moment, will always refer to the expected productivity when completed of the investments now in the process of construction. Or, in other words, it will be based on the assumption of a certain rate of investment continuing for some time in the future. Only investments in excess of those required to complete those already under way will bring us to a point on the transformation curve to the right and below *P*, while continued investments over a certain period and at a decreasing rate are required even to keep us at the point *P* which had already been reached. Should investments fall below the amount required to carry on with the processes already commenced, this would shift us to a point above and to the left of *P*. But the position into which such an unexpected decline of the rate of investment would place us would not be the same as that in which we would find ourselves had we never expected investments to be larger than they now turn out to be. The expectation of a larger supply of investible funds (or of a lower rate of interest) has had the effect that the investments commenced have been given a form different from what would otherwise have been the case; and although the marginal percentage returns on the larger amount of capital which was expected to be invested would have been smaller than those on investments requiring a smaller total amount, the fact that the former have already been commenced means that the whole return to be expected from them depends now on the further investments required to complete them, and can, if necessary, be attributed to those completing investments. In other words, the existence of "uncompleted investments" (i.e. investments which can be made to produce their full output of consumers' goods only if further investments are made) creates a specially urgent demand for capital, and will tend to raise the marginal productivity of investment much above the level at which it would have been if the supply of capital had never been expected to be higher.

The point *P* in the diagram then stands for a position at which *TB* of the potential current income (which otherwise might have been permanently maintained at the magnitude *TO*) was being invested and further investments sufficient to complete those under way were expected to take place. The marginal productivity of investment, represented by the coincident slopes of the two curves at *P*, would be the expected productivity of the completed investments. Had investments never been expected to be at the rate of *TB* per period, but only at the rate of *TA*, we should instead be at point *P′*, where the slope of the transformation curve is little smaller than at *P*. But if, after we had already moved to *P*, the rate of investment were unexpectedly reduced to *TA*, we could not move back to *P′* along the given transformation curve. The situation would have been altered by the investments already made in the expectation of larger future investments, and the capital actually available, instead of being sufficient to enable us to start further new investments at a slightly lower rate of return, is now not even sufficient to complete those already started. The transformation curve along which we could then alone move would be something like the dotted curve *PT″*. If the supply of capital fell to *TA*, the marginal productivity of investment would therefore rise to a figure indicated by the slope of the tangent at *P″*.

We cannot assume, however, that with such a change in the marginal productivity of investment the supply of capital will fall as much as it would have done if the marginal productivity had remained constant. Indeed, we are not entitled in this context to think of the supply of capital as a fixed magnitude but ought to represent the change in the willingness to save as a shift of the indifference curves. Let us assume, therefore, that what has happened is that the indifference curves have shifted so that instead of the fully drawn curve II' the dotted curve I_2I_2' is now tangential to the original transformation curve at the new point P'. But as we can move backwards only along PT'', the " kink " at P will mean that even after a very considerable shift in the position of the indifference curves, tangency between the new transformation curve and one of the indifference curves ($I_1\ I_1'$) will still occur at the point P. In other words, within the limits determined by the angle of the transformation curve at the " kink ", the marginal productivity of investment will be determined exclusively by the slope of the indifference curve, or by " time preference "; and even above P the transformation curve is likely to be so strongly curved that the indifference curve may be comparatively flatter and therefore determine the marginal productivity of investment !

The conclusion we must draw, therefore, would seem to be that so long as we consider solely an evenly progressing economy, the marginal productivity of investment will depend almost exclusively on the investment demand schedule with the supply of capital adapting itself to the newly constant rate of productivity. But as soon as this even progress is held up, and the supply of capital turns out to be less than had been expected, " time preference " takes charge—or, since " the fall in the supply of capital " is merely another form in which we refer to a rise in " time preference ", so long as time preference remains constant or falls, the productivity element will be dominant; but whenever " time preference " rises, it takes control and we may get as a result sudden and violent increases in the marginal productivity of investment, and, in consequence, of the rate of interest.

To translate all this from the language of " time preference " and rates of transformation into that of demand and supply of future consumers' goods on a market presents no real difficulty, but would occupy more space than we can give to it at the present moment.

Some Implications of the First in–First out Method of Stock Valuation

By K. Lacey

Some discussion has recently been centred around two conceptions of stock valuation, referred to succinctly as ' the value of the movement ' and ' the movement of the value ', respectively. The former is, I suppose, favoured by most economists; the latter is the method used in practice by accountants. The former method is employed, *inter alia*, in ascertaining stock accretions for purposes of statistics of national income and investment; it involves computing the current cost value of the movement in the volume of stock. The latter is concerned with calculating the profits of individual firms, and part of this profit is represented by an increase in the value of stocks since the date of the previous accounts.

The movement in the value of stocks as shown by actual accounts is not the same as the value of additions to quantity because under the present accounting system the whole of the opening stocks are regarded as sold and as replaced by new stocks at different cost prices. The movement in value is therefore composed of two elements :—

(*a*) The cost value of increased quantities, and

(*b*) The difference in the cost of new stocks replacing the volume of stocks at the beginning of the year.

The accounting method which produces this result is known as the First in–First out method, i.e. sales are regarded as made out of the earliest acquisitions so that closing stocks are all regarded as late acquisitions at their own cost price. If it were desired to adopt the conception of stock valuation implied in the phrase ' the value of the movement ', it would involve changing to an accounting method under which sales would be regarded as satisfied out of the latest additions, so that replacements in the volume of stock would remain at a constant price.

The ' movement of the value ', therefore, means a First in–First out (*FIFO*) basis of valuation, and the ' value of the movement ' a Last in–First out (*LIFO*) basis. It will be convenient to use these terms *FIFO* and *LIFO* below.

In this brief article we shall consider some of the economic implications of the existing *FIFO* basis of valuation not already discussed. The argument is based on the consideration that under the *FIFO* method unit stock values rise during a boom and fall during a slump, with a consequent inflationary and deflationary effect upon business profits and capital gains, and the further result of over- and under-spending respectively. Of this tendency to over- and under-spending, I can, as an accountant, speak with some assurance, but the comments below on some of the economic implications should be regarded mainly as suggestions for consideration by economists. Income based upon a *LIFO* valuation of stocks will be called the ' actual ' income because the figures are not distorted by paper profits or losses arising out of unit stock value movements; income based upon a *FIFO* valuation will be called the ' apparent ' income.

I

One important effect of the fluctuation in the spending out of profits and capital gains arising out of the *FIFO* method is likely to be a variation of wages in the opposite direction. This is borne out by statistical evidence[1] which shows that the percentage of total income taken as wages rises during the later period of the slump and falls during the boom. The idea that the difficulties of cyclical recovery are caused by a resistance to monetary wage reductions would appear to be the reverse of the truth. If during a depression the effect of the *FIFO* method is that traders and investors spend a smaller proportion than usual of their share of actual income, the fact that the share going as wages and salaries shows an upward tendency is an advantage at least in the sense that further accumulations of unsold stocks are avoided. Ignoring for the moment the possibility of traders and investors spending more during the slump, it is the resistance to the upward tendency of the share of actual income going as wages (e.g. by reductions in money wages, a change to a less progressive tax system, a time lag in price reductions, etc.) which impedes recovery.

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[1] See, for example, page 94, *National Income and Outlay*—Colin Clark, and article in the *Economic Journal*, September, 1939, " Real Wage Movements," by Professor J. H. Richardson. Hourly real wages should normally move in the same direction as the movement of the *apparent* share if the assumptions of the present article are correct.

Statistics do not usually reveal the operation of this resistance because they are usually based on income computed under the *FIFO* method, i.e. they show the apparent and not the actual income. But Colin Clark in his national income computations has at least made the correction to a *LIFO* basis in the case of falling stock values, though he has not appreciated the need for a similar adjustment in the case of a rise. However, his figures on page 94 of *National Income and Outlay* covering the distribution of income for the years 1928–1932 deal with a period of falling prices, and show the actual income. He brings into his aggregate income figures, not the movement in the value of stocks, which is the method employed by traders in their accounts, but the value of the quantitative movement in stocks. He thus shows the division of income calculated on the basis of keeping intact the real capital.

This adjustment to income in respect of stock valuation reveals quite clearly that during the early part of the depression, i.e. the latter part of 1929 and the year 1930, the share of actual income going as profits and interest was increasing and that the share received as wages and salaries was falling. But traders and investors were not under this impression; on the *FIFO* basis of stock valuation their apparent profits showed a substantial reduction both in amount and in the percentage of apparent national income. The table on the next page gives both Colin Clark's figures of the division of actual national income 1928–1934 and the apparent division under the First in–First out method.

The deflationary effect of this stock valuation discrepancy is obvious from the table. Not only was there a large percentage of actual income set aside as stock depreciation and not spent, but the reduction in the apparent share (after stock depreciation) received as profits tended to create a pressure towards reduced money wages, etc., which exacerbated the total under-spending and drove prices still further downwards. It was only when wages received a substantial increase in the actual share of income, and when falling profits and unemployment had reduced the ratio of savings in 1931 and 1932 that the way was paved for recovery by a stabilisation of prices.[1] Subsequently the share going as wages declined with the disappearance of the gap between apparent and actual profits following price stabilisation. Later on still, in the period 1935–1937, there was a gap in the opposite direction, apparent profits being in excess of actual profits as prices rose, but the description of the effects during the downward movement will be sufficient to enable readers to fill in the analysis of the upward movement for themselves.

It is not difficult to see how the present basis of stock valuation creates tension and disequilibrium. Wage earners are dissatisfied with their reducing share of income as now computed during the upward phase, and traders are even more strongly dissatisfied with the apparent increase in the share of labour and the reduction of profits, as now calculated, during the depression. An alteration of accounting methods to a *LIFO* basis would assist in avoiding this tension. The share going to wages would be steadier because the amount spent by traders would tend to show less fluctuation, and as a consequence price variations would not be so wide or so cumulative. But it would appear to be necessary to make the change as from the time of a relatively low price level; if traders have over-spent (or what is more relevant at the moment, have been over-taxed) during the upward movement of prices, they can hardly be expected (even if it were financially possible) to avoid putting the matter right by under-spending when prices fall.

II

So far we have been concerned with the effects of First in–First out methods of stock valuation upon wages. That, it would seem, is the more important aspect;

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[1] The 1929/30 reduction followed by a 1931/32 increase in the share was also the experience in U.S.—*vide* figures quoted by Lord Keynes, p. 48, *Economic Journal*, March, 1939.

COMPARISON OF 'APPARENT' AND 'ACTUAL' INCOME DIVIDED BETWEEN FACTORS OF PRODUCTION 1928—1935

	1928		1929		1930		1931		1932		1933		1934		1935	
	£ mill.	Per cent.	£ mill.	Per cent.	£ mill.	Per cent.	£ mill.	Per cent.	£ mill.	Per cent.	£ mill.	Per cent.	£ mill.	Per cent.	£ mill.	Per cent.
Wages—																
Actual	1479	42.3	1486	41.8	1434	40.5	1366	42.8	1333	42.5	1362	42.0	1442	41.5	1520	40.5
Apparent	1479	42.6	1486	42.8	1434	43.6	1366	44.3	1333	43.2						
Salaries—																
Actual	922	26.4	944	26.6	898	25.3	885	27.8	890	28.3	907	28.0	922	26.5	937	25.0
Apparent	922	26.5	944	27.2	898	27.3	885	28.7	890	28.8						
Profits and Interest—																
Actual	801	22.9	821	23.1	902	25.5	620	19.5	590	18.8	642	19.8	781	22.4	949	25.4
Apparent	778	22.4	740	21.3	651	19.8	518	16.8	539	17.5						
Rents—																
Actual	294	8.4	302	8.5	308	8.7	316	9.9	325	10.4	329	10.2	334	9.6	339	9.1
Apparent	294	8.5	302	8.7	308	9.3	316	10.2	325	10.5						
Total Actual Income ..	3496		3553		3542		3187		3138		3240		3479		3745	
Less Stock Depreciation.	23		81		251		102		51							
Total Apparent Income..	3473		3472		3291		3085		3087							

but it is necessary also to consider the opposite position, namely, the effects of wage variations upon First in–First out stock valuations.

On the whole it would appear that market price considerations arising from the demand side are more important in determining unit stock values than wage rates. The fall in stock values in 1929–1932 was not the result of reduced wages but of reduced demand, and similarly the rise in unit stock values in 1935–1937 was more a reflection of rising market prices than of increased wages. This situation arises because stocks in all stages of production except the first are valued at the market price cost of acquisition from the trader in the preceding stage. Only a relatively small proportion of unit stock values is composed of the factor cost of direct wages, etc., i.e. it is limited to the factor cost of that particular stage and of that particular trader. Even in the primary production stage, stocks are usually valued at market prices insofar as they are covered by forward sales contracts.

Nevertheless, to the extent that monetary wages rise during a boom and fall during a slump and at the same time form a direct part of unit stock values, they operate in the same direction as other tendencies, i.e. they inflate or deflate stock values as now computed and create over- and under-spending. If in valuing stock we ignored variations in price and took into account only the current cost of variations of volume, wage rates would influence stock values only in respect of current unit costs imputed to additions to quantities; the inflationary or deflationary effects of wage variations would then be relatively small.

III

The next question to be discussed is the effect of *FIFO* methods of stock valuation upon prices. It is mainly the influence through supply prices with which we are concerned, and this varies according to commodity, stage of production, state of competition, etc. The *FIFO* basis implies that each sale is satisfied out of an item of stock acquired or produced one, two, three, or more months before, according to the size of the stock carried in relation to sales. In selling the item in question the trader normally bases his supply price upon its cost, which means not the present cost, but the cost at the time it was acquired or produced a month or more beforehand.

In the relatively inelastic raw material industries, the producer is to a large degree at the mercy of demand fluctuations and exposed to the risk of violent price movements. He cannot, for example, hold out for a price based upon *FIFO* costs when the market falls. He can, however, and to a large extent does, cover himself against price fluctuations in the future by selling his production on forward contracts and thus passes on the risk to the dealer or manufacturer. But this does not protect him from vagaries of buying on the part of manufacturers, etc., which provide him with a relatively handsome yield above cost at one time, and a yield falling below cost at another.

The producer is largely at the mercy of the unstable demand of the manufacturer. The manufacturer in turn is at the mercy of consumer demand, although the amplitude of variation is much less pronounced. The effect of a reducing consumer demand during a depression is inexorable, but it is slower and later in time than in the raw material industries. Manufacturers can and do succeed to some extent in basing selling prices, though at a reduced margin, upon *FIFO* costs, and this applies particularly where competition is not so keen (e.g. protected home industries), or where an agreed price policy is in some measure operative. It is not difficult to see how this resistance to or lag in price reduction relative to current costs tends to sustain a depression. (It was, for example, the main cause of the increased actual share going to profits in the years 1929–1930.) Similarly, during the boom, the upward movement of selling prices, despite increased profit margins over *FIFO* costs, tends to lag behind the increase in current costs and so feeds the expansion. These cumulative upward and downward movements have an aggravated effect upon raw material producers, a fluctuation in consumer demand creating a magnified fluctuation in the demand for raw materials and at a far greater range of price than in the consumer goods industries; we are thus back to the point at which we started. If prices could be based on current costs, stocks being valued on the *LIFO* basis, the present instability could be eased considerably. When raw material prices rose, the rise in the price of the finished product would follow more quickly and without so much lag, yet profits as computed under this method would be smaller than at present. Under conditions of falling prices for raw materials, the price of the finished product would fall more rapidly, yet profits would be at a higher level than under our present accounting system. We are here assuming the movement of price as given, but the effect of the change of basis, if adopted widely, would be to smooth out price fluctuations. Over- and under-optimism in acquiring raw materials should then be checked more speedily and the wide range of raw material prices should be reduced considerably.

IV

The previous section has a bearing upon the fact, mentioned in Sir William Beveridge's *Full Employment in a Free Society*, that certain export trades appear to lead both in depression and recovery. This suggests that the movement of market values of raw materials may be the initial cause of the boom and slump. When prices fall, for example, the raw material producing countries find their purchasing power curtailed, not only in terms of money, but in real terms, because prices of manufactured articles are reduced much more slowly and with a time lag. Consequently, exports to those countries fall in volume at a greater rate than the fall in volume of world trade as a whole. After a time, however, selling prices of finished products are brought into a more normal relationship to raw material prices, and then exports cease to fall and tend to rise in advance of a general recovery. If prices of manufactured articles could be based upon current costs instead of *FIFO* costs, fluctuations of raw material prices would be reflected by a corresponding fluctuation in the price of the finished product, and the present disparity would tend to disappear.

V

As already stated, the above comments are merely suggestions for consideration which appear to be implied in the use of the *FIFO* method of stock valuation. They appear to fit in with our experience of the working of the *FIFO* method, and to encourage a belief in the need for an alteration to a *LIFO* basis. It will, of course, be understood that only short term trends are under consideration and that long term influences also affect the actual position. The failure of real wages in U.K. to rise in the years 1896–1914, for example,[1] may be due to the long term influence of the increase of overseas investment during that period, a possibility which Professor Richardson does not mention. At all events, further statistical study, including the correction of past statistics in respect of stock valuation, is necessary before a clear picture is available.

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[1] Mentioned by Professor J. H. Richardson, p. 432, *Economic Journal*, September, 1939.

The Distribution of Family Incomes

By E. C. Rhodes

When statistical data relating to income distributions are examined, it is often found that the " incomes " which are the units are themselves aggregates. There may be included husband's and wife's earned and unearned incomes. It is possible that the distributions of these different kinds of incomes may not be the same. It is possible, under certain simple hypotheses, to determine the distribution of aggregate incomes.

Let us consider a simple case of the distribution of an aggregate of two incomes u and v. Let us assume that the distribution of income u is given by $f(u)\,du$ and that of income v by $\phi(v)\,dv$, where u may range from u_0 to infinity, and v from v_0 to infinity. We want to know what is the distribution of the aggregate $x = u + v$, where x will range from $u_0 + v_0$ to infinity. The number of aggregate incomes at or above x is $\iint f(u)\,\phi(v)\,du\,dv$, where the integration ranges from values of u and v determined by $u + v = x$, x being fixed, to infinity.

If we call $\int_u^\infty f(u)\,du = F(u)$ and $\int_v^\infty \phi(v)\,dv = \Phi(v)$, this integral may be written in the form $F(u_0)\,\Phi(v_0) - \int_{v_0}^{x-u_0} \phi(v)\,dv \int_{u_0}^{x-v} f(u)\,du.$

This is $F(u_0)\,\Phi(x-u_0) + \int_{v_0}^{x-u_0} \phi(v)\,F(x-v)\,dv.$

A simple example will illustrate this method. Suppose $F(u) = e^{-\mu(u-u_0)}$, $\Phi(v) = e^{-\lambda(v-v_0)}$, where $F(u_0)$ and $\Phi(v_0)$ are both unity.

Then the number of aggregate incomes at or above x ($= u + v$) is $e^{-\lambda(x-u_0-v_0)}$ $+\int_{v_0}^{x-u_0} \lambda e^{-\lambda(v-v_0)}\, e^{-\mu(x-v-u_0)}\,dv.$

This becomes, after some reduction,

$$\frac{1}{\lambda-\mu}\left(\lambda e^{-\mu(x-u_0-v_0)} - \mu\, e^{-\lambda(x-u_0-v_0)}\right).$$

Thus, if the constituents of the aggregate income are exponential curves, the distribution of the aggregate income is the difference between two curves of this kind.

Again, suppose $F(u) = \left(\frac{u}{u_0}\right)^{-p}$, $\Phi(v) = \left(\frac{v}{v_0}\right)^{-q}$. Then the number of incomes at or above x is

$$\left(\frac{x-u_0}{v_0}\right)^{-q} + \int_{v_0}^{x-u_0} q\,\frac{v^{-q-1}}{v_0^{-q}} \left(\frac{x-v}{u_0}\right)^{-p} dv,$$

or

$$v_0^{q}\,(x-u_0)^{-q} + \int_{v_0}^{x-u_0} q\,u_0^{p}\,v_0^{q}\,(x-v)^{-p}\,v^{-q-1}\,dv.$$

Let us write $\frac{v}{t^2} = \frac{x}{1+t^2} = \frac{x-v}{1}$.

Then the expression is

$$v_0{}^{q}\,(x-u_0)^{-q}+2\,q\int_{\sqrt{\frac{v_0}{x-v_0}}}^{\sqrt{\frac{x-u_0}{u_0}}} u_0{}^{p}\,v_0{}^{q}\,x^{-p-q}\,\frac{(1+t^2)^{p+q-1}}{t^{2q+1}}\,dt \quad . \; . \; . \quad (1)$$

Let us consider a simple case when both p and q are equal to 1.5.
The number of incomes at or above x is

$$v_0{}^{1\cdot 5}\,(x-u_0)^{-1\cdot 5}+3\int_{\sqrt{\frac{v_0}{x-v_0}}}^{\sqrt{\frac{x-u_0}{u^0}}} u_0{}^{1\cdot 5}\,v_0{}^{1\cdot 5}\,x^{-3}\,\frac{(1+t^2)^2}{t^4}\,dt\ .$$

This is

$$v_0{}^{1\cdot 5}\,(x-u_0)^{-1\cdot 5}+3\left\{\sqrt{\frac{x-u_0}{u_0}}-\sqrt{\frac{v_0}{x-v_0}}-2\sqrt{\frac{u_0}{x-u_0}}+2\sqrt{\frac{x-v_0}{v_0}}\right.$$
$$\left.-\tfrac{1}{3}\left(\frac{u_0}{x-u_0}\right)^{1\cdot 5}+\tfrac{1}{3}\left(\frac{x-v_0}{v_0}\right)^{1\cdot 5}\right\}\frac{u_0{}^{1\cdot 5}\,v_0{}^{1\cdot 5}}{x^3}.$$

After some reduction, this may be written

$$n=\frac{u_0{}^{1\cdot 5}}{x^3}\left(\frac{x^2+4xv_0-8v_0{}^2}{\sqrt{x-v_0}}\right)+\frac{v_0{}^{1\cdot 5}}{x^3}\left(\frac{x^2+4xu_0-8u_0{}^2}{\sqrt{x-u_0}}\right)$$

, taking n as the number of incomes at or above x.

We may also find $w=\int_x^\infty n\,dx$. We get, in this case, $w=2\,\frac{u_0{}^{1\cdot 5}}{x^2}\sqrt{x-v_0}\,(x+2\,v_0)$ $+\,2\frac{v_0{}^{1\cdot 5}}{x^3}\sqrt{x-u_0}\,(x+2\,u_0)$. (Note $w=z-nx$ where $z=\int_x^\infty y\,x\,dx$, n being $\int_x^\infty y\,dx$.)

We know that w/nx is a useful statistic for income distributions; it is constant for all x when the distribution is of the Pareto type, $n=x^{-p}\,x_0{}^{p}$.

For purposes of illustration we will simplify our case still further, taking $u_0=v_0$, and we get $\frac{w}{nx}=2.\,\frac{(x-u_0)\,(x+2\,u_0)}{x^2+4\,x\,u_0-8\,u_0{}^2}$. Thus $\frac{w}{nx}$ changes with x. As x increases from $2\,u_0$, which is the lowest possible value of x, $\frac{w}{nx}$ declines from the value 2 to a minimum 1.5, when $x=4\,u_0$, and then increases gradually to the value 2 again when x is infinite. Thus for the higher values of x, w/nx is an increasing function.

We find, from actual data, many income distributions for which the value of w/nx continuously declines in the higher income ranges. This simple case we have just considered does not correspond to any data we have yet encountered.

Other examples of the expression (1) so far dealt with do not appear to correspond with factual data.

We have not proceeded further with the consideration of the case where the original income distributions were exponential curves, because we wished to concentrate on distributions of the Pareto type, these being, it was felt, most like those which occur in practice. It is true that the exponential types do produce statistics w/nx which behave similarly to data which have been examined, i.e., decreasing with increasing x, except that in the exponential case w/nx tends to become zero when x is large, whereas our data show that this statistic is different from zero for large x.

These considerations, and others which are now to be examined, lead us to adopt a different line of approach. It is felt likely that when two or more constituent incomes are involved in the aggregate income, there will be some degree of correlation between the constituents. Arguments may be put forward of this nature. If a person has a large earned income, he will tend to save large amounts and on investment enjoy a large unearned income. Also, it is possible that a person enjoying a large unearned income may have many opportunities of finding himself in a position where he can earn a large income. Alternatively, it may be said that a person with a large income may be tempted to accept only unpaid work. It may be argued that a man with a large income is more likely than a man with a small income to meet a woman with a large unearned income; thus there may be a tendency for husband's and wife's incomes to be positively correlated. On the other hand, one may argue that the man with a modest income having married a rich wife will devote his energies in other directions than remunerative labour. On balance, we think there is some likelihood of there being a positive correlation between two constituent incomes forming an aggregate of the kind we are dealing with. On this assumption, we will proceed to the next problem.

At the outset, we are faced with the difficulty of writing down a correlation function of the type we wish to consider. We want to preserve the Pareto character of the distribution, but simple correlation functions are very few. We have used a very simple one which, it is admitted, is unlikely to conform to facts, but the results achieved are suggestive and for this reason we proceed further.

We assume that there are two constituent incomes u and v, as before, and that the combined distribution of incomes is $\frac{ab\,p\,(p-1)\,du\,dv}{(au+bv)^{p+1}}$, the distribution of u ranging from u_0 to infinity, and v from v_0 to infinity.

The distribution of u alone is $\frac{a\,(p-1)\,du}{(au+b\,v_0)^p}$, and that of v alone is $\frac{b\,(p-1)\,dv}{(au_0+bv)^p}$. These distributions of u and v are not Pareto distributions of the original type since they each include a constant term in the denominator; but they are modified Pareto distributions.

The correlation between u and v is measured by the correlation coefficient r which is $\frac{1}{p-1}$, and the correlation is linear.

Now dealing with the aggregate $x = u + v$, we find that the number of incomes at or above x is given by $n = \frac{1}{a-b}\left(\frac{a}{(b\,x+\overline{a-b}\,u_0)^{p-1}} - \frac{b}{(ax-\overline{a-b}\,v_0)^{p-1}}\right)$. The expression for

$$w = \int_x^\infty n\,dx \text{ is } w = \frac{1}{(a-b)\,(p-2)}\left\{\frac{a}{b}\,\frac{1}{(b\,x+\overline{a-b}\,u_0)^{p-2}} - \frac{b}{a}\,\frac{1}{(a\,x-\overline{a-b}\,v_0)^{p-2}}\right\}.$$

Let us confine our attention to a very simple case, when $p = 3$. Here $r = \frac{1}{2}$. Let us call $u_0 = 1$ and $v_0 = 1$.

$$n \text{ becomes } \frac{1}{a-b}\left(\frac{a}{(b\,x+a-b)^2} - \frac{b}{(a\,x-\overline{a-b})^2}\right),$$

$$w \text{ becomes } \frac{1}{a-b}\left(\frac{a}{b}\,\frac{1}{(b\,x+a-b)} - \frac{b}{a}\,\frac{1}{(a\,x-\overline{a-b})}\right).$$

$$\text{Then } \frac{w}{n\,x} = \frac{\big((a^2+ab+b^2)\,x-(a^2+b^2)\big)(a\,x-a+b)\,(b\,x+a-b),}{ab\,x\,\big((a^2+ab+b^2)\,x^2-2\,x\,(a^2+b^2)+(a-b)^2\big)}$$

and when x is very large this tends to 1. Generally, when x is large $\frac{w}{n\,x} \to \frac{1}{p-2}$.

In order to illustrate the behaviour of $\frac{w}{n\,x}$ for different values of x, let us complete our example by taking $a = 11$, $b = 1$. We then have, $\frac{w}{n\,x} = \frac{(133\,x-122)\,(11\,x-10)\,(x+10)}{11\,x\,(133\,x^2-244\,x+100)}$.

The table below gives values of $\frac{w}{nx}$, together with values of the same statistic computed from Sur-tax statistics for England and Wales, 1931–32.

VALUES OF w/nx

Theoretical Distribution		Sur-tax statistics, 1931–32	
x	w/nx	Income Limits £	w/nx
20	1.501	2,000	1.495
25	1.401	2,500	1.408
30	1.334	3,000	1.356
40	1.250	4,000	1.279
50	1.200	5,000	1.234
60	1.167	6,000	1.206
70	1.143	7,000	1.177
80	1.125	8,000	1.153
100	1.100	10,000	1.116
150	1.067	15,000	1.081
200	1.050	20,000	1.046
250	1.040	25,000	1.046
300	1.033	30,000	1.033
400	1.025	40,000	1.010
500	1.020	50,000	0.993
750	1.013	75,000	0.985
1,000	1.010	100,000	0.859

There is some likeness between the two series of w/nx in this table, though there is not exact correspondence. We took the value $p = 3$ in our illustration because the values of w/nx from the Sur-tax figures seemed gradually to tend to a value in the neighbourhood of unity. The values of a and b, 11 and 1 respectively, made w/nx take the value 1.5 approximately for $x = 20$. Our illustration appears to supply us with a series of values of w/nx which behave in a manner similar to the observed behaviour of these statistics derived from the data. Further, if we now compute the appropriate n's and w's from our formulæ for the various x's we get figures like the sur-tax figures, as may be seen in the table below.

VALUES OF n and w

Theoretical Distribution			Sur-tax statistics, 1931–32		
x	n	w (tens)	Income limits £	n	w (£000)
20	94,000	282,191	2,000	94,087	281,391
25	69,080	241,899	2,500	68,515	241,192
30	52,898	211,674	3,000	51,938	211,327
40	33,861	169,352	4,000	33,192	169,811
50	23,517	141,132	5,000	23,043	142,163
60	17,279	120,974	6,000	16,920	122,427
70	13,230	105,854	7,000	13,055	107,570
80	10,454	94,094	8,000	10,400	95,900
100	6,998	76,988	10,000	7,059	78,684
150	3,308	52,931	15,000	3,350	54,331
200	1,920	40,329	20,000	1,986	41,548
250	1,251	32,573	25,000	1,285	33,598
300	881	27,320	30,000	910	28,201
400	504	20,657	40,000	527	21,289
500	326	16,606	50,000	343	17,035
750	146	11,144	75,000	154	11,381
1,000	83	8,385	100,000	97	8,334

It will be remembered that in our example we took u_0 and v_0 both equal to 1. In order to make our x correspond to actual income limits we must regard x as measured in £100 units. Thus u_0 and v_0 will correspond to £100.

We may, therefore, say: if we assume that aggregate incomes are made up of two parts, each distributed in a modified Pareto manner, the lowest possible income in each case being £100, and that there is a correlation of 0·5 between the constituent incomes, we shall get a distribution of aggregate incomes in the higher income ranges which is distinctly like the distribution of incomes liable to sur-tax in England and Wales in 1931–32.

We have examined the values of w/nx for the England and Wales Supertax and Sur-tax statistics from 1910–11 to 1937–38. The change in w/nx with x is not exactly the same each year, though there is general agreement in so far that w/nx decreases as x increases in these high income ranges. The illustration used here will not correspond with distributions for other years. We are merely suggesting, at the moment, that possibly a fruitful method of dealing with the analysis of high income distributions involves starting with the initial assumption that the incomes we are dealing with are made up of at least two parts which may be subject to different laws of distribution, and that there may be a positive correlation between the parts.

Book Reviews

Civitas Humana: Grundfragen der Gesellschafts und Wirtschaftsreform. By WILHELM RÖPKE. Eugen Rentsch Verlag. Zurich. 1944. 408 pp. Fr. 13.50.

Civitas Humana is a continuation and expansion of Professor Röpke's earlier book, *Die Gesellschaftskrisis der Gegenwart* (reviewed in ECONOMICA, Vol. XI, No. 43, p. 151), in which with much skill and sincere eloquence he set forth the case for a conservative liberalism. Röpke is no apologist for historical *laissez faire* capitalism, some of his criticisms of which are even more sweeping and far-reaching than those fashionable among socialists, and he would prefer to restrict the use of the term capitalism to describe only that corrupt and degenerate form of the market economy which developed in the 19th century. The preservation of the market principle, purged of the excrescences which in the past have hampered its beneficent work, he regards, however, as the most fateful question for our civilisation, for this in his view is the essential economic condition for the preservation of a healthy liberal democratic society, and he rejects out of hand the possibility of any compromise between it and collectivism, or *Kommandowirtschaft*, which from its very nature must be based upon compulsion. The battle must be fought on two fronts, the sharpest criticism of historical capitalism being combined with equally energetic opposition to any collectivist intrusion into the market economy, and active state intervention to eradicate monopoly influences wherever they may show themselves.

In *Civitas Humana* Röpke further elaborates his argument in two main directions. His ultimate objective is not exclusively or even mainly economic ; for him the realisation of certain positive social-philosophic ends is much more important, and economic organisation is merely an instrument for their attainment. He has frequently protested against the dangerous consequences of a too rigid demarcation between the social sciences: sound economic doctrine, he maintains, is impossible unless economists pay more attention than at least in the recent past many of them have been accustomed to do to the necessity of integrating the economic side of society and social development with other aspects more fundamental for the attainment of the good life in the widest sense. Accordingly he examines the spiritual and intellectual foundations of the present world crisis, dealing faithfully with the overweening pride of hyper-rationalism, which so often leads to disillusionment, scepticism and anti-intellectualism, and with the errors of Positivism and *Szientismus*, with which readers of ECONOMICA will already be familiar. He then proceeds to examine the conditions upon which the maintenance of a healthy State depends, the miracle whereby the many accept the orders of the few without perpetual strife, and in particular the counterpoises, the men of learning, religion, the legal system and the press, whose efficient functioning will provide adequate protection against any excessive extension of state power, and to analyse the pathological social symptoms of proletarianisation in a mass society which in his view afflict many who would not readily recognise themselves as proletarians, and which now constitutes such a dangerous threat to sound social development.

This sociological analysis is followed by a partial answer to the impatient critics who have reproached Röpke for failing to produce any clear cut "programme" of reform. To them he answers with some plausibility though not complete conviction, that such a far-reaching and complex social disease as now afflicts us is not amenable to treatment according to any neatly contrived "plan". The important thing is in dealing with each particular manifestation of the disease to keep steadily in mind the wider objectives whose full realisation must be slow and gradual, the preservation of conditions in which a market economy may function effectively, the encouragement in every possible way of decentralisation and the "deproletarianisation" of modern society. Anti-monopoly policy is accordingly not treated here in any great detail, and a more systematic study of the international aspects of the problem is promised for the future. A good deal of attention is, however, given to the social value of property, and also to some of those aspects of Röpke's economic philosophy which appear strangest in the eyes of English liberal readers, and in particular to the strategic position to be assigned in a healthy society to the craftsman and independent worker, and to the revival of peasant agriculture.

Röpke feels that we are now at a crucial turning point in human history, and have before us a unique opportunity to make a decisive choice which will save us from the fatal consequences of the accumulated errors of the last century. His whole argument is developed with a sense of urgency and a persuasive eloquence which cannot fail to command the respect even of those who do not take the view, upon which Röpke insists, of the significance of the organic relationship between its several parts, and who are not convinced of the validity of all its details. Röpke indeed seems to suggest that all the implications of his social and economic philosophy as elaborated by him must be accepted and applied as a whole. Many English readers will find it difficult to accept this conclusion without some radical qualifications, and with a different background of experience will still be inclined to insist on their right to pick and choose. To the convinced continental liberal, the apparent opportunism of the average English economist, and his propensity for voting for parties which at least claim to be socialist, has always been rather a puzzle, and economists who have grown up in the more distant Dominions, where state action of one kind or another has become so much a commonplace that in many contexts it appears to raise no very profound or significant question of principle, but to be merely a matter of convenience, are even less ready to accept all of the clear cut distinctions to which Röpke attaches so much importance. We may indeed have carried our opportunism too far, and with increasing experience of the unpleasant way in which one thing leads on to another, may now perhaps wisely decide that the time has arrived when more clear cut distinctions between ideologies are called for. But though a Dominion economist may feel that Australia and New Zealand are still far from a definitive solution of the fundamental problems of society, and many of the modern manifestations of social growth in these lands may be positively distasteful to him, it will nevertheless take a good deal to persuade him that these imperfections are the inevitable consequence of the decisions taken many years ago to build railways as a state enterprise or to permit the state to operate in other fields from which a doctrinaire liberalism would have excluded it.

Röpke's sceptical view of the value of elaborate blue prints will command a good deal of sympathy, but at the same time one feels that it does not justify the almost complete failure to indicate even the first practical steps towards the realisation of some of his more radical objectives. This difficulty arises in an acute form in connection with the modern urban conglomerations which fill Röpke with such profound horror, and the dissolution of which he regards as a *sine qua non* for a reformed and civilised society. He believes that even to-day there is no reason why the maximum size for a well-balanced and healthy city, adequately discharging all its functions, should exceed 50 to 60 thousand inhabitants. One may, it is to be hoped, appreciate fully the abnormalities of modern metropolises without wishing to destroy them entirely. Röpke's views on this point show indeed how difficult it is, even when one is well aware of all the dangers involved, to avoid the insidious attractions of absolutes. At least one of Röpke's readers feels himself to be a thoroughly urbanised person (a characteristic, it should be added, which by no means involves either a lively interest in the radio or skill in driving an automobile), with not the slightest desire to own a plot of ground or cultivate his own garden, with very little of the will to property which Röpke regards as an essential basis for membership of a sound society, but with a strong sense of the advantages of being able from time to time to walk down a main thoroughfare with no risk of meeting a familiar face. He is however reluctant to believe that these characteristics necessarily disqualify him from effective participation in the great debate in which Röpke plays such a redoubtable part. It might almost be suspected that, despite his emphatic rejection of the mechanical uniformities which he fears collectivism threatens in the modern world, and his emphatic and sincere appreciation of the ultimate value of spontaneous variety, he was himself afflicted by a secret ambition to insist that all-righted people should share his own tastes and prejudices. We should not perhaps too lightly reject the hint sometimes thrown out that those who do not agree with him are thereby demonstrated already to be so profoundly infected by the diseases for which he is seeking a remedy that their judgment is no longer to be trusted. But this is clearly a hard doctrine, the acceptance of which would itself mean the abandonment of the sturdy independence of mind which Röpke would readily agree was another valuable asset for a healthy society.

Röpke's work affords heartening evidence that in the free corners of Europe thinkers have already during the war been grappling manfully and effectively with the fundamental issues which the present conflict compels us to face. His books have moreover evoked a response in Italy and Hungary which suggests that some of the thought current in this country about " inevitable " post-war trends in countries with recent experience of German centralised control may need drastic revision, and even from Germany there is evidence that experience has induced some to return to " liberal " conclusions. But while *Civitas Humana* will be highly appreciated by English readers who are already sympathetic with the fundamentals of Röpke's outlook, they may feel a little embarrassment in using it as an instrument for the conversion of those who do not share his faith in the fundamental value of the market principle. For they might be obliged to spend so much time in explaining that in their view many items on his agenda which he regarded as essential were in fact side-issues or irrelevant, that there might in the end be considerable difficulty in keeping attention focused on the main points.

Röpke's abhorrence of mechanical conformity, and of mechanical interpretations of social integration, his alarm at the lack of civic courage and weakening sense of responsibility on the part of many who should give a lead to the society of which they are members, his criticisms of the fruitless modern search for absolute guarantees of security, and of current tendencies which treat the individual as little more than a cog in the wheel whose tastes are to be disciplined and actions controlled in conformity with some preconceived plan, his insistence that it is more important to remould our economy so that it fails to produce gross inequalities of wealth rather than to allow the inequalities to emerge and then to attempt to redress the balance by redistribution, his healthy scepticism about the " inevitability " of the growth in every direction of giant industrial units, all constitute a body of doctrine which evokes a warm response from the English liberal. But he finds it difficult to believe that the acceptance of these views inevitably carries with it, as Röpke seems to imply, an obligation to insist that, as a father, he should have an additional electoral vote, or to believe in the virtues of Second Chambers or peasant societies, or to be suspicious of the Beveridge social insurance proposals as a typical product of mass society. Similarly one can appreciate the social value of independent workers, and regret the obsession of many modern economists with large-scale industry, leading them to comparative neglect of the economic significance of the vast mass of miscellaneous services which in a modern economy may already account for half of the aggregate of employment, without being convinced that we must insist on independence of this kind as the ideal for everybody. It is probable that success in efforts to break down the restrictive barriers which impede the expansion of production and maintain anti-social inequalities may bring to life an increasing number of the small units and independent workers for whom Röpke longs. But it might be wiser to accept them for the most part as by-products of an economic policy based mainly on other objectives rather than to make them our direct and avowed objective.

Röpke indignantly and perhaps justly refutes the charges of romanticism which have been levelled against him, but there is in his thought more than a colour of nostalgic longing for an idealised past. Obviously we must not be content merely to take things as we find them. The remedies for the evils of modern urban society are, however, to be sought mainly within urban society rather than by attempting to turn it back into a rural society. There is no doubt a good deal to be said for Henry Ford's notion, of which Röpke heartily approves, of combining factory work with sparetime cultivation of the soil, but it would be a sad (and quite unnecessary) expression of defeatism to suppose that only on this condition is civilised life possible for a factory worker. It is something more than English opportunism or pragmatism which justifies the view that if by some miracle an ideal society could be realised it would probaby present an even more variegated appearance than Röpke seems to think proper.

ALLAN G. B. FISHER.

The American Patent System. By WILLIAM B. BENNETT. Louisiana State University Press. 1944. vi + 259 pp. $3.00.

There has been a good deal of questioning recently, on both sides of the Atlantic, of the merits of the Patent system. Its advocates maintain that patents promote technical progress, by encouraging invention, by inducing investment which would not be forthcoming under fully competitive conditions, and by discouraging secrecy. Its critics maintain that the spur of competition and the desire to keep a jump ahead of rivals is quite sufficient to promote invention and investment in the development of new commodities and processes, that secrecy is in any case impossible to preserve over a wide range of processes, and that the patent system plays into the hands of large monopolies which on balance retard rather than promote progress. This is one of those unsatisfactory controversies which it is impossible to clinch with clear evidence one way or the other. Mr. Bennett comes down on the side of patents, after a somewhat perfunctory treatment of the opposition case.

One of the admitted evils of the present system is the existence of a large number of patents which, if challenged in the Courts, would seem likely to be declared invalid, but which go unchallenged because of the expensiveness of litigation. Under the British system it is not open to the Patent Office to refuse a patent because the idea it discloses is lacking in invention. The American Office has power to refuse a patent on these grounds, but no satisfactory definition of invention has ever been formulated, and many close fought actions in the Courts in both countries show that the riddle : When is an invention not an invention ? is by no means easy to answer. Even when the Patent Office sets a certain standard of invention, plenty of room for doubt remains. The fees required under the British, but not under the American, system to keep a patent alive act as a sieve to sort out worthless patents, but a patent may be worthless from the point of view of its contribution to technical knowledge and still have value as an instrument of monopoly. Neither the British nor the American system succeeds in confining the grant of patents only to genuinely useful inventions. Mr. Bennett points out the desirability of raising the standard of patents, but makes no promising suggestions for bringing this about.

Under English law, the abuse of monopoly rights under a patent (for instance, by failing to manufacture the article concerned) may be countered by compelling the patentee to grant someone else a licence to work the patent. No such provision exists in the American law, and Mr. Bennett takes the view that all this sort of thing is best left to the Anti-Trust Laws. The Report of the National Patent Planning Commission, however, which evidently appeared after Mr. Bennett's book was completed, recommends that in matters which affect public health, public safety or national defence, a patentee should not be able to prohibit an infringer from continuing production. This would have much the same effect as a provision for compulsory licensing in the field concerned.

Mr. Bennett wraps a good deal of cottonwool round the thorny question of international cartels based on patents, and, indeed, the whole book gives the impression of having been written rather for the sake of writing a book rather than from any strong urge to penetrate into the very difficult questions which it raises.

JOAN ROBINSON.

Omnipotent Government. By LUDWIG VON MISES. New Haven, Yale University Press. 1944. ix + 291 pp. $3.75.

Broadly stated, the main argument of this book is that State intervention in the free market is bound to be undertaken on behalf of members of a particular national group and is therefore bound to produce friction in the international sphere, leading ultimately to the danger of war. The argument will not be strange to readers of Professor Mises' other works, and the present book may perhaps be regarded as an elaboration of the chapters in *Die Gemeinwirtschaft* on the Foreign Relations of a Socialist Community. It is illustrated by a depressing but convincing account of the decline of economic liberalism in Germany.

The case seems to the present reviewer to belong to the large, and unhappily growing class of economic truths which people neglect at their peril. It is plain enough in Anglo-Saxon countries where Government intervention in the economic process tends more and more to take the form of the distribution of favours to particularly economic groups at the general expense. It is glaringly and tragically obvious in continental Europe where long before the present war intervention took the form of economic discrimination against linguistic or religious minorities within the same international boundaries. Those who hope that a new and better world will emerge out of the plans of highly nationalistic governments to improve the economic position of (their own) national groups, may be in for some serious disappointments. The effect of the war (indeed one of the chief means of bringing it to a victorious end) has been to exacerbate nationalism and to strengthen and elaborate the economic controls of the belligerent national Governments. The prospect of lasting international harmony emerging out of this situation may be visible to the eye of faith; it is certainly undiscoverable to the eye of reason.

Professor Mises' knowledge of the historical period which covers the decline of liberal ideas in Europe may be said, without offence, to be extensive and peculiar. His explanation of the fact that the linguistic groups of Central and Eastern Europe interpreted nineteenth century liberalism simply as national self-determination and "jumped" from monarchical absolutism to totalitarian forms of government without going through the liberal democratic stage at all, is suggestive and satisfying. His interpretation of the failure of the Weimar Constitution and the collapse of the German Communists in the 1930's is very much to the point. "The Weimar democracy was of a peculiar sort. The horsemen of the Kaiser's Guards had fought for it and won it."

The line of thought in *Omnipotent Government* is closely akin to that in Professor Hayek's *Road to Serfdom*, though with more emphasis on developments in Germany since Bismarck and with more positive suggestions for European economic reconstruction on liberal lines. It does not, however, meet the critics of economic liberalism so squarely on their own ground as Professor Hayek has done. "Nobody succeeded in the attempt to reject the demonstration that no economic calculation is possible in a Socialist system," says Professor Mises (p. 282). This may be true, but Dickinson, Lange, Lerner and others seem to deserve a more explicit answer than this. "Government spending is not an appropriate means to brush away unemployment" (p. 65). This also may be so, but the oceans of ink which have been spilt on this subject since the last war demand more extensive notice than two or three short paragraphs.

Those who swim against the strong intellectual tides now running have a heavy task indeed. They must defend their own beliefs in the current idiom of their opponents, and they must restate received truths acceptably to a generation grown sceptical of them. Professor Mises does not really meet the Keynesian point that the free system will not work without self-generating disturbances. He does not explain what exactly is meant by a "free market", by "private property", or by "free enterprise". Lack of precision on some of these points was a serious shortcoming in the writings of the early liberal economists, and it is time that this state of affairs was remedied by their intellectual successors. Professor Mises is a distinguished member of this select body. His book shows tellingly that the present trend towards economic interventionalism is liable to lead to disastrous international conflict. This is only half the task. He ought now to tell us precisely what a "free market economy" means, what sort of government action is necessary to sustain it and enable it to meet the shocks which theory and practice alike show to be inevitable; and what are the practical stages by which it can be finally reached in a world which, for the last thirty years, has been moving ever faster in the opposite direction.

A. S. J. BASTER.

Public Spending and Post-war Economic Policy. By SHERWOOD M. FINE. New York: Columbia University Press. Oxford: Humphrey Milford. 1944. x + 177 pp. 16s. 6d.

This book is, perhaps, best considered as an interim report on the problem of recasting our ideas about the rôle of public works in a world turned upside down. It is probably too early to achieve anything much more clear-cut, and Dr. Fine may be congratulated on an enterprising effort. He will be the first to agree that the topics which he has broached will, as the reconstruction period becomes imminent, require to be further explored with all the resources of pooled experience and cooperative planning.

Dr. Fine is a disciple of Professor Hansen, and although he submits the "secular-stagnation" doctrine to considerable criticism in detail, his book is coloured by the vision of an increasing deficiency between the employment offered by private enterprise and the aggregate labour resources of the community. His outlook in this respect gives rise to a philosophy summarised in the last few sentences of the book, where he concludes that "The relative scope of public to private investment will be determined in large measure by the extent to which contemporary business and labour practices, among other things, are revitalised. Whether the host of pitfalls confronting the resurgence of private enterprise can be successfully surmounted remains to be seen."

It is to the problem here indicated that Dr. Fine chiefly addresses himself, but neither its practicability nor the prospect of its success emerges very clearly from his pages. A considerable part of his argument is, in fact, devoted to a reconsideration, from this standpoint, of the effects of the fiscal policies of the New Deal; on which he reaches the conclusion that they were vitiated by a lack of appreciation of the real mechanism of the multiplier, complicated by undue sensitivity towards the economy-complex of Congress. This, however, has been better done elsewhere —notably in Professor Villard's *Deficit Spending and the National Income*—and the ideas which Dr. Fine contributes do not receive sufficiently acute and detailed examination to leave any very clear impression. But the rôle of pioneer in economic thought is an ungrateful one, and a later and clearer insight may come to attach to this book a greater significance as a pilot light towards the problems of the new world than it appears to-day to have to a perhaps rather blasé reviewer.

J. K. HORSEFIELD.

The Shifting and Incidence of Taxation. By OTTO VON MERING. The Blakiston Company, Philadelphia. 1942. xiii + 262 pp. $3.25.

This is a book in the Seligman tradition; the search (as stated) is for the " locale of the final burden of a tax ". It sets out from the point where the revenue authorities pitch on a particular group of producers and extract a tax from them. These then proceed to throw the tax, as it might be a tennis ball, at the consumers. If the latter hang on to the ball then our search is at an end; the tax has " come to rest " in Seligman's phrase. But the consumers may throw it back again, or, alternatively, in the first instance the producers may have thrown it not to the consumers (a forward shift) but to another group of producers (a backward shift); and so the rally goes on, always conceived in terms of a definite piece of tax, whose presence can be detected by an equivalent price rise.

This type of analysis may possibly be illuminating when it is applied to some taxes on particular forms of outlay, but it is quite unnecessarily complicated, and even where it is reasonably appropriate it is open to serious objection, not least the unreal dichotomy between the initial stages of the operation of a tax (the shifting process) and the later stages (its effects). Both Cannan and Edgeworth pointed this out in the nineties. When applied to general income and capital taxes the tennis ball approach breaks down hopelessly, and Dr. von Mering finds himself groping about (in company with several of the Colwyn Committee witnesses) in the search for prices that have risen as a result of an increase in income tax. It never strikes him to look first at the general phenomena of spending, saving and investing.

Dr. von Mering's main interest appears to be in diagrammatic exposition, and he has made an exceptionally complete collection of the diagrammatic arguments which have been put forward at one time or another. So far as he seeks to draw any conclusions from his diagrams they run in terms of the extent to which " production will be affected " in particular industries, according to the respective slope and curvature of the supply and demand curves. Dr. von Mering does not believe that incidence analysis can assist directly in the choice of taxes; so far as he advances any criterion of choice, it would seem to lie in the avoidance of those taxes which reduce saving.

URSULA K. HICKS.

Origins of Academic Economics in the United States. By MICHAEL J. L. O'CONNOR. Columbia University Press. Humphrey Milford. 1944. 367 pp. English price: 28s.

There is high authority for the reminder that we do well to look to the hole of the pit whence we are digged; it may be, however, that only the professional geologist should examine it with the over-conscientious care displayed in this volume. Mr. O'Connor's work is a study in origins, a contribution to economic palæontology in which many fossils, some more interesting than others, are disinterred, catalogued and described.

Mr. O'Connor is throughout concerned with the teaching of Economics rather than with the substance of Economics; and indeed his subject is primarily the teaching of economics in the North-eastern colleges and schools of the United States, as embodied in what he calls ' the Clerical School '. On his way to his main subject he disposes of other rival schools, of which three are distinguished. There is firstly the Jeffersonian School, relying largely on Destutt de Tracy, with a somewhat radical philosophy and a considerable animosity against Banks; secondly there is the protectionist school represented by Matthew Carey, father of the better-known H. C. Carey; and thirdly, a school identified with Cooper, with anti-clerical tendencies and an unbecoming frankness regarding labour conditions. As against these tendencies, " the acquisition of conservative associations by political economy was requisite for the acceptance of the subject in the academic halls of the north-eastern states ". Political Economy had to become a sedative, rather than a stimulant. Inevitably in the early days of the teaching of Political Economy, reliance had to be placed on European books, but, as Mr. O'Connor remarks, " the clerical school found much of the foreign political economy lacking in sufficient reference to the power of the Deity ".

A survey of the books used is in some respects not without interest. Adam Smith was not much used; and indeed no one would claim for *The Wealth of Nations* that it is, or ever was, a satisfactory text-book. Mrs. Marcet's masterpiece was a very considerable influence—" though puerile in its form, and from a female pen," in the condescending words of one American commentator. The chief foreign text, however, was Say; and it is a revealing fact that in 1880 Say was still going strong, and that (as here recorded) his book had attained by that time twenty-six American printings as against eight in France. McCulloch, as edited and annotated by McVickar, was another of the leading European influences.

Naturally in the fullness of time the Clerical School produced their own interpretations, more suitable (as was supposed) to American conditions. Two somewhat ponderous chapters survey the ' Clerical Versions ' and the ' Clerical Primers ' designed for use at lower educational levels. What was desired was a Political Economy which would illustrate divine wisdom. Some of the quotations given by Mr. O'Connor are calculated to bring a blush to the hardened brow of the modern economist. Political Economy, we learn in one place, is " that philanthropic science which, next to the gospel, whose legitimate offspring it is, will do more than anything else for the elevation and fraternisation of our race ". In these sections Mr. O'Connor gives more than all anyone need know about Newman, and Sedgwick, and Vethake, and Wayland (probably the most successful of them all), and about many more.

Although there are gleams of interest, and although there is an admirable Bibliography and a somewhat overwhelming list of text-books, no one would contend that this is a book which can be read with ease or pleasure, or even with much ultimate profit. Mr. O'Connor has been over-conscientious; he has done his task too thoroughly. He presents, at considerable length, all his chief exhibits, and after the lapse of approximately a hundred years, this procedure produces in the minds of all but the most attentive readers an impression of repetition. This impression is accentuated by Mr. O'Connor's practice in adding, to each chapter, a Summary somewhat over-generous in its amplitude. Nor is any detail too small for Mr. O'Connor. He can tell the reader the relative amount of space devoted to the various parts of the subject in the text-books under discussion. It is somewhat disconcerting to be informed, for example, that in the smaller Wayland the proportions are : " Introduction, 4 per cent.; Production, 35 per cent.; Exchange, 31 per cent.; Distribution, 18 per cent.; Consumption, 12 per cent." He must indeed be a morbid text-book fan to whom such details make an appeal.

ALEXANDER GRAY.

Sound Policies for Bank Management. By ROBERT G. RODKEY. New York: The Ronald Press Company. 1944. xiv + 224 pp. $4.00.

This book offers to the American small-town banker of the nineteen-forties something comparable in content, though not in plan, to that classic handbook of earlier banking generations, George Rae's *The Country Banker*. It is instructive to compare the two and to notice how much the emphasis has shifted in sixty years: from judgment of men to scientific analysis of accounts; from the duty of the banker towards his community to his duty *vis-à-vis* his depositors; from the tacit assumption of a steadily rising volume of deposits (save only during a " run ") to a balance sheet total which ebbs and flows with the Trade Cycle; from the risks of discounting an embarrassing flood of commercial paper to the problems of adjusting commitments when no such paper is available. And it is perhaps equally significant to see how extremes meet, and how, in the reinforced concrete home of Professor Rodkey's Hypothetical Bank, no less than in the Oxborough Branch of the District Union Bank, " old and mean-looking, and rather out of sight ", the same fundamental problems of fractional reserves have to be faced.

To Professor Rodkey, however, the rule of thumb methods advocated by George Rae are outmoded; he would substitute a scientific array of primary and secondary reserves, related not only to the current volume of deposits of different kinds and origins, but to the probable drop in these deposits during the next slump, as measured by their fall in the last one. He throws in, also, argument and advice on technical problems not only of banking practice proper, but of accountancy, such as the best method of dealing with realised bond profits and the importance of publishing adequately informative statements of the Bank's position.

To some extent, necessarily, Professor Rodkey's advice applies peculiarly to small banks or to American conditions—witness not only the repeated references to a " run ", but also the suggestions on the education and activities of the Board of Directors, and such omissions as that of suggesting any limits to real estate loans, because these are already prescribed by local legislation. To some extent, also, his proposals appear to the outside observer slightly inadequate—for example, the proposal (p. 46) to compensate for a supposed lack of secondary reserves by increasing primary reserves in the form of balances with other bankers, which seems (to mix metaphors) to encourage banks to give hostages to fortune by taking in one another's washing.

But in most directions the instructions given are sensible and constructive. Thus (to cite only one instance which I have noted) valuable emphasis is placed on the importance of systematically determining the limits within which short-dated stocks must be preferred, for their protection against capital depreciation, to longer-dated securities, despite the latter's better yield. This is, in fact, a book which any banker could read with profit, and which does credit to its academic origin.

J. K. HORSEFIELD.

Central Banking Functions of the United States Treasury, 1789–1941. By ESTHER ROGOFF TAUS. New York: Columbia University Press. Oxford: Humphrey Milford. 1943. xii + 313 pp. 23s. 6d.

" We are all Socialists now," at least to the extent that responsibility for monetary policy is generally accepted as being a function of Government, and not of an independent Central Bank. But there is less unanimity on the residual duties of Central Banking. Some would limit these to holding the spare cash of Government and of the commercial banks, managing the National Debt, and supplying banking expertise to advise on and participate in the day-to-day adjustment of the supply of currency to the demands for it. Others would add responsibilities in the field of foreign exchange, and others again would postulate some supervisory duties in connection with less impeccable banking units. In these directions complete agreement is not to be looked for, if only for the reason that conditions in no two countries are sufficiently similar to enable their Central Banks to perform identical functions.

To the reader with the foregoing predispositions, however, it is a somewhat curious experience to encounter a full-dress study of the Central Banking field which still attributes to autonomous financial institutions the unfettered scope of nineteenth century *laissez-faire* conceptions. For Mrs. Taus a Central Bank is the appropriate arbiter and controller of monetary policy and currency practices. Her thesis, meticulously documented and interestingly elaborated, is that throughout the history of the United States it has been the Treasury, and not any part of the banking system, which has occupied that position.

The mechanism by which monetary control has been enforced has varied, as Mrs. Taus clearly shows, both in scope and in technique. The relations of the Treasury with the money market have, for example, been sometimes direct and sometimes conducted at one remove through the First or Second Bank of the United States or through the Federal Reserve System. There have been variations, too, due to the alternating affluence and penury of the Treasury; and still others arising from the differing personalities, policies and economic predilections of the Secretaries of the Treasury themselves. That, despite these vagaries, the Treasury "has from the beginning been a deciding factor in the banking field" is due, as Mrs. Taus shows, to the fact that "it has always been in the unique position of being the largest receiver and disburser of funds". To the English reader it seems manifest that this preponderance has itself been chiefly caused by the atomistic structure of American banking; given that structure, it appears probable that effective Treasury control, at least before 1914, was inevitable. But Mrs. Taus has performed a valuable task in showing just how this control grew up, and what were its conditions and limitations.

The analysis of the latter-day balance of power between the Treasury and the Federal Reserve System, and the assessment of their relative weight as policy-forming agencies, is considerably more difficult. Here Mrs. Taus' treatment is, at least to a reader unfamiliar with the subtle nuances of American politics, somewhat less convincing. She is more handicapped by a certain naïvety, perhaps more of expression than of analysis, which appears to derive from her rather ultramontane conception of Central Banks' functions. A more realistic conception of what, in the circumstances, say, of 1933, was a Central Bank's duties, and what was necessarily the expression of Government policy, might have turned these chapters from a historical record into a contribution of importance towards our understanding of the proper functions of politics in the monetary field.

J. K. HORSEFIELD.

The Settlement of Industrial Disputes. By KURT BRAUN. The Brookings Institution. Blakiston Co., Philadelphia. 1944. xii + 306 pp. $3.50.

German students and ex-civil servants whio have found pressing reasons for leaving their country during the last ten years are adding considerably to the output of literature on social and economic questions, especially in America. Many of them are learned men and most of them have a nostalgic affection for some of the institutions of their late homeland. Mr. Kurt Braun is one of these. His book shows a careful study of the industrial scene in America, but it breaks little new ground. It is in fact a book for the elementary student who is not quite sure what an industrial dispute really is and who never heard of collective bargaining or conciliation.

Mr. Braun writes as a sociologist who sees the need for a stable theoretical foundation for industrial relations in the modern world. And he is doubtless right; loose thinking on these matters is all too common. But when he comes to constructive proposals for Americans to follow, it is back to the defunct Weimar Republic that he turns for his model. Certainly the labour courts and labour judiciary of that German era were well planned and highly developed. They deserve mention in a wistful historical note, but they are assuredly not the stuff for America or even for Britain. They were far too legalistic for our turbulent communities. Probably they made it easier for the Nazi regime to carry state intervention to its logical conclusion, i.e. the supercession of conciliation and collective bargains by the dictates of an official called a Labour Trustee. These men, with the backing of the Nazi Labour Front, settle everything with a high hand and each controls a large economic area.

Mr. Braun cannot draw a very tidy picture of conciliation and arbitration in the U.S.A., but he is an intelligent observer, and has done his best to trace a thread of orderliness running through the chaos.

RONALD DAVISON.

The Problem of Valuation for Rating. By J. R. HICKS, U. K. HICKS and C. E. V. LESER. National Institute of Economic and Social Research: Occasional Papers No. VII. Cambridge University Press. 1944. 90 pp. 7s. 6d. net.

The disparity in the level of assessment of property for rating purposes is a matter of common knowledge. This disparity arises not only between one district and another, which is not surprising as valuations are still made on a local basis and the Central Valuation Committee has only an advisory function, but also between different properties in the same district. The present paper, which is the second of a series of three papers incorporating the results of an enquiry into the burden of Local Taxation undertaken by Professor and Mrs. Hicks on behalf of the National Institute of Economic and Social Research (Mr. Leser's share in this particular paper is limited to the statistical analysis), sets out to analyse the extent and causes of variations in the level of valuation and to make certain suggestions for its reform.

In a sense this paper deals with what, from the point of view of the original enquiry, is a side issue, but it is nevertheless an issue of considerable importance affecting as it does the " fairness " of the rates both as between individuals in the same district and individuals in different districts. The first of these three papers, published last year under the title *Standards of Local Expenditure* (the third is still to come), dealt with the burden of the rates from the point of view of the variations in the standard of expenditure between different districts: the third paper will deal with the other important aspect of the burden of the rates, namely the incidence of the rates in the sense of the proportion of individual incomes they represent. This second question depends on the proportion which gross rent (i.e. inclusive of rates) bears to total income, and to the proportions in which gross rent is divided between rent proper and rates. If rates were always proportional to rents, this latter proportion would depend on the rate poundages, but as is well known rateable values often bear little relation to rent. It is, therefore, necessary to know something about rating valuations as a preliminary to the main task of analysing the incidence of the rates. This is the problem with which the present paper, which is based on the results of an enquiry conducted by the Ministry of Health in 1937–38, is mainly concerned, but consideration has also been given to the wider question of the reforms which may be desirable in the present valuation system.

There are two major aspects of the problem of valuation. In the first place, differences in valuation as between different properties in the same rating area affect the distribution of the rate burden between individuals in that area. This is by far and away the most important consideration as it is immaterial, as between individuals in the same district, whether valuations are made on a high or a low basis, provided the same basis is applied consistently. The authors find the greatest measure of undervaluation in the case of post-1918 houses, particularly the small post-1918 houses. This conclusion is one of considerable importance, indicating as it does where the pinch would be felt if valuations were levered up to the full rental basis prescribed by the law. It is also, incidentally, an indication of the real value of an analysis of this nature based on accurate and detailed information. The view often held is that it is the old pre-1914 property which is lightly assessed, as indeed it is in some instances. The present report demonstrates that taking the country as a whole this is the reverse of the truth.

Secondly, variations in the level of valuation between districts affect both the contribution to the County Rate and the receipt of Block Grants from the Exchequer. As the authors point out, this is not an issue of the same magnitude, although the tendency for wealthy districts to be relatively undervalued and for poor districts to be overvalued favours the wealthy district as against the poor; moreover the relatively high level of valuation in poor districts, by keeping the poundages lower than they otherwise would have been, does to a certain extent prevent adequate recognition of the plight of these districts.

A word of criticism. In dealing with the effect of rating valuations on Income Tax valuations, the authors make the statement that " Schedule A valuations now follow valuations for rating " (p. 69). Since the introduction of the Excess Rents provisions in the 1940 Finance Act, Income Tax has been payable upon the full rent received in the case of all property which is let (subject of course to allowance for repairs, etc.). In the case of owner occupied property, there is admittedly a tendency for the Schedule A valuation to follow the rating valuation, but this is by no means always the case. The reactions on the Income Tax are, therefore, by no means as serious as the authors suggest.

F. A. COCKFIELD.

Un Programme Social. By ADRIEN TIXIER. *Le Problème administratif français.* By GERMAIN WATRIN. Office français d'édition. Algiers. 1944. 37, 77.

The break in intellectual communication between France and England since 1940 has given to the few French books which reached England the personal interest of letters from friends in the occupied countries. It was and still is the works of pure literature whose beauty and sincerity have brought the greatest relief and excitement to English friends of France. Of the political and social thinking of Frenchmen under the occupation we still know very little.

These two brief pamphlets which reach us from Algiers are concerned, from different points of view, with the whole problem of the social, economic and political organisation of the Fourth Republic. The first is a report of an address, delivered in October, 1943, by M. Adrien Tixier (now a member of the de Gaulle government, and at that time "Commissaire pour les affaires sociales" in Algiers) to a meeting organised by the "Union départementale des syndicats confédérés d'Alger". The second, by M. Germain Watrin, Professor of "Droit public" at Beirut, is, in the main, a historical account and defence of the various methods by which, under the Third Republic, public utilities were operated by organisations designed to combine, in the interests of the consumer, the virtues of state control and private enterprise (*concession, régie intéressée, société d'économie mixte*). In the constructive suggestions for the future, which are the slightest part of the essay, M. Watrin looks forward to a continuation and perfecting of such methods, which would involve no far-reaching measures of social change.

M. Tixier's address envisages a far deeper transformation in French society, and looks forward to a political and social order which will secure "the right to work, the right to really humane conditions of labour, the right to social security" of the whole body of French workers. He suggests in a few paragraphs, based, he tells us, on reports received from the resistance leaders, the creation of "regional councils" for the control of industry, which are to include representatives of the State, the trade unions and "on occasion" (*le cas échéant*) of the employers' associations; these councils to be subject to a national economic council whose composition and functions are not clearly defined.

In both these essays, addressed to the general reader, and making no claim to an exhaustive treatment of their vast subject, the English reader is struck by a note of sobriety and tolerance, and by a general approach which is reminiscent rather of the mental habits of his own countrymen than of the uncompromising adherence to general political principles, combined with a certain distaste for the shady game of practical politics, which he had learnt to associate with many of the finest French intellects before the war. The principles of "liberty", "order", "property" and "democracy" are thought of as quantitative rather than absolute conceptions; and both writers appeal, as an overriding principle, to a conception of "public service" and the "general interest of the country" which there is no attempt to subject to rigid logical analysis. The effect of both these pamphlets, more especially of M. Tixier's reference to the many and varied plans sent to him by the resistance leaders, of which he cannot "give a detailed analysis", is to sharpen our desire for the renewal of complete interchange of thought and experience with the new France.

M. WALLAS.

Report of the Advisory Committee on Reconstruction. Edmond Cloutier. Ottawa. 1944. 64 pp.

I. *Final Report of the Sub-committee on Agricultural Policy*. Edmond Cloutier. Ottawa. 1944. 48 pp.

II. *Final Report of the Sub-committee on Conservation and Development of Natural Resources*. Edmond Cloutier. Ottawa. 1944. 29 pp.

III. *Final Report of the Sub-committee on Publicly-financed Construction Projects*. Edmond Cloutier. Ottawa. 1944. 35 pp.

IV. *Final Report of the Sub-committee on Housing and Community Planning*. Edmond Cloutier. Ottawa. 1944. 339 pp.

V. *Final Report of the Sub-committee on Post-war Employment Opportunities*. Edmond Cloutier. Ottawa. 1944. 27 pp.

VI. *Final Report of the Sub-committee on Post-war Problems for Women*. Edmond Cloutier. Ottawa. 1944. 34 pp.

These Reports were commanded to advise the Canadian Government as to post-war policy. The Government established three Advisory Committees for the purpose: on Demobilisation, Reconstruction, and Economic Policy. Here we have the Report of the second of these Committees, and the Final Reports of its six Sub-committees.

The Committee candidly states that its Report "does not constitute a definitive blue-print for the post-war policy of the Dominion, since it is our unanimous opinion . . . that further study of each of the specific problems discussed herein must be undertaken by the appropriate departments of Government . . . as well as by interested private groups." This is indeed true of all the Reports, except that on

Housing. It is therefore enough, apart from the Housing Report, to indicate their broad drift and contents. They are self-described as sign-posts; and sign-posts rather evade criticism.

The Advisory Committee's Report gives the main outlines of the problem. The impact of the war has artificially hastened Canada's industrialisation. We learn with a start that " the Canada of to-day is definitely more urban than rural ". We are not therefore surprised to find that her problems are very like our own—employment, social security, foreign markets, need for surveys of public investment projects, need for new forms of labour bargaining controls. So far we are on familiar ground. But we also note that Canada is suddenly to be a creditor instead of a debtor country: and as she must still depend heavily on wheat exports, in view of her established farm organisation, the problem is underlined. A whiff of the Canada we have imagined reaches us when we read of her natural forest, water and wild-life resources. The pioneer tang is in the air when the need is stressed for aerial surveys, properly assigned to the R.C.A.F., over her mighty spaces. The young giant is still growing naturally, though his stirrings towards industry have been over-stimulated by war.

The Report on Agriculture (I) goes more into detail. We face such facts as these. Two-thirds of Canadian wheat went abroad: and bacon to Britain in war conditions has expanded four times. Tariffs on flour had already been a worry. And, failing bulk contracts, bacon may shrink as rapidly. So the Committee naturally turns to the need for research, on which it notes that Canada has been backward. And it is dogmatic that never again must prices fall to the levels of the early thirties. In agriculture also, then, Canada seems to re-echo some of our programmes. Indeed, even on the prairies, overcrowded farm houses have been endemic.

The Report on Natural Resources (II) responds most to our imaginations. But here also, war has acutely interrupted development in favour of war goods. If Canada must now cultivate her own garden more intensively, the resources are certainly there. For this, wide and accurately detailed surveys are needed. And organisations to train the men and apply their skill—to water power, minerals, forest, wild-life, fisheries, soil—are imaginatively conceived and firmly outlined. It is refreshing to find that the prospecting fervour of individuals should be fostered: though planning still keeps breaking through in a training scheme for thousands of prospectors. Evidently these Canadians agree with Marshal Stalin that the individual prospector remains the quickest pioneer. Especially as to wild life, the report gives us a sense of cultured wisdom as well as economic sagacity. A Board should study the fluctuations of the animals, and Government photographers should give us motion-pictures—an alluring prospect.

The Report on Public Projects (III) is interesting for the use made of experience in the U.S.A. It is also encouraging to note that this Committee actually recognises the existence of the *economic* problem. We are so tired of lists on lists of truly admirable projects—five per cent. of which would exhaust our free resources. This Committtee at least recommends that priorities must be definitely defined; and the order must also be based on ascertained costs. Some attempt is even made to evolve a model tabulation for costs, and to analyse them in the interconnections of a regional scheme.

The Report on Post-war Employment Opportunities (V) also covers much ground we know too well. The need for mobility is stressed over this wide Dominion. But there is freshness for us in the approach to apprenticeship questions and training for the semi-skilled. These schemes should be arranged by and pooled within the *industries*, under representative direction. *Verb. sap.*

The war has also inevitably intensified problems of Women in Industry (VI). The attitude to family allowances has elements of realism, not overstressed in Britain: allowances up to a maximum of six children are considered: and some supervision is suggested in necessary cases. As to new jobs also fresh hints are drawn from New Deal policies. But the root of difficulty remains the low wage group.

The Report on Housing (IV) is on a different level. It has all the weight, detail and authorised judgment of a Royal Commission Report. It at once becomes a basis for all future work on Canadian Housing. The need is clear. The problems are of the British order of magnitude. Canada needs from six hundred thousand to a million houses in ten years. Here again there is not much new in the way of principle. British readers will note with some diffidence that we are described as pioneers in this sphere. This may be so in principle. But all who know the facts know also how far our housing and health standards remain paper aspirations. However, just because this Report goes into Canadian facts and details, many points of comparative interest arise for us.

There is, for instance, an interesting examination of income groups relative to the proportions of income spent on housing—a line which could be usefully pursued everywhere. The large family issue is also freshly and sensibly treated. It is realised that the conventional house checks larger families. And it is soundly deduced that if we want these families we must find out how many there are likely to be if the houses are provided; and we must then supply these houses. Of course, any estimate of this probable desire for—and achievement of—large families is difficult, even for our planners. An elastic house graded like progressive taxation seems called for. But the *approach* is right. And it is emphasised by the estimates of overcrowding, which are a shock to our ideas of spacious Canada. The figures and proportions are (perhaps not surprisingly) almost on Scottish levels; though the standard of adequate housing is higher than ours—one to a room.

It is also intriguing to find that in Canada housing fluctuations between the wars went *cyclically*. Comparison with British facts is inevitable. But the more one looks at it, the more treacherous easy conclusions become. Especially, a high proportion of Canadian houses is wooden. Have the Canadians that attitude to houses as funds of stable wealth which holds for British stone and lime and bricks? Only on the latter view would one invest specially in buildings in slumps. It is also suggested that the fall in our long interest rates after 1932 may have had more *unique* effects because of its very bigness, than we have been apt to assume. Again, the argument from our improved terms of trade and their support to our building boom receives some support from the opposite sequence in Canada.

There is much of similar interest in this Housing Report. But enough has been said to indicate its quality and scope.

We in Britain cannot learn too much of these virile and well-loved sister nations. So these reports are entirely welcome.

A. L. MACFIE.

The Economics of Full Employment. Six Studies in Applied Economics prepared at the Oxford University Institute of Statistics. By T. BALOGH, F. A. BURCHARDT, M. KALECKI, K. MANDELBAUM, E. F. SCHUMACHER, G. D. N. WORSWICK. BASIL BLACKWELL. 1944. vii + 213 pp. 12s. 6d.

In presenting this volume six economists connected with the Oxford Institute of Statistics have joined to elucidate various aspects of "Full Employment". Such a joint enterprise, of course, has, first of all, to solve the problem how to prevent overlapping. This is here successfully achieved by means of the interesting device of putting the main theoretical weight of the book into two of the essays and leaving the task of subsidiary explanation to the four others. Accordingly, the two star performers are Dr. Balogh and Mr. Kalecki. Their two papers are genuine contributions to economic science, closely reasoned, always interesting, mostly original. The junior partners in the enterprise have been left to embroider the garment, to embellish it with attractive ornaments, and, in general, to instruct the public in up-to-date ways of wearing it. It is therefore hardly surprising that the various parts of the volume are somewhat uneven in quality.

The book as a whole centres round three problems which may be briefly labelled as "redistributive taxation", "the wage-price spiral", and "Full Employment in one country?"

Mr. Kalecki prefers taxation as a means of raising the marginal propensity to consume to the budget deficit method. Any harmful effects of taxation on the inducement to invest are to be obviated by a "modified" form of income tax. "Imagine that income tax is charged on gross income, *i.e.* before deduction of wear and tear. On the other hand all investment in fixed capital, whether for the sake of replacement or expansion, is deducted from the taxable amount." (46). In other words, all income invested in fixed capital is free of tax. This ingenious proposal creates difficulties where firms borrow capital for investment. Who, we may wonder, is to benefit, the lender or the borrower? And would it require much ingenuity to devise methods by which income invested in one year would be "lost" the next year?

In pursuing this line of thought, Mr. Schumacher finds scope for his remarkable journalistic gifts in painting a gloomy picture of the consequences of continuous budget deficits. The danger here is not, however,—as the unsophisticated might have thought—that the public, losing confidence, may refuse to take up government bonds. The danger, according to Mr. Schumacher, is that the growth of the public debt may create a situation in which no further deficits are required! "It may ultimately lead to an undesirable rise in the number of rich rentiers and to an equally undesirable increase in conspicuous consumption". That is to say, the propensity to consume of the wealthy may gradually rise to such an extent that their savings—and consequently the very need for further deficit finance—disappear. This would indeed solve the problem of unemployment, but it would give to "full employment" a content which could hardly be defended on moral or social principles." (103)

But "income redistribution" is also required to solve the wage-price problem. While some economists have recently seemed inclined to shirk the issue, Mr. Kalecki boldly faces it. His solution of the problem how to prevent continuous pressure on the wage level from generating inflation, consists in price stabilisation by means of subsidies financed from taxes on non-wage incomes. "Wage bargaining is likely to change its character in full employment. It will have to be linked with 'bargaining' for higher income tax on larger incomes and thus will relate the policy of trade unions more closely to general economic policy." (55)

This suggestion is then taken up by Mr. Worswick. "Under this scheme real wages would rise continuously, in proportion to money wages, while the real 'available' income of non-wage earning classes would be correspondingly reduced by taxation. In addition, the budget deficit would also be continuously reduced. If the rise in money wages is not too rapid, the method might indeed be said to combine the virtues of gradualism, fiscal orthodoxy, and social justice!" (66). Alternatively, we may squeeze profit margins by more direct methods. "To the extent to which entrepreneurs cannot compensate for rising wages by reducing costs, the price stop must bring about a redistribution of gross incomes (*i.e.* incomes before taxation); there will be a shift from profits proper to wages. This will lead to an increase in effective demand, since the propensity to consume of wage-earners is higher than that of capitalists. Progressive taxation must therefore be increased (or the deficit spending on public investment reduced) to bring effective demand back to the full employment level. The working of the price stop is thus rather complicated, and difficult to forecast." (67). It is, however, far less difficult to forecast that a continuous rise in wages with prices "pegged" will sooner or later eliminate all profits and thus ultimately lead to a fall in output and consumption. For in the absence of profit no amount of "effective demand" will call forth effective supply. The argument clearly rests on the implicit assumption that all non-wage incomes, not only profits, are residual and therefore indefinitely "squeezable". But is this true? Neither forensic eloquence, we should have thought, nor sartorial elegance, are independent of the level of incomes of those who supply us with these services.

"The International Aspects of Full Employment" are treated by Dr. Balogh. In effect he demonstrates that a World Economy based on international division of labour becomes impossible if some countries follow a "full employment policy" while others rely on private investment. For the more fully integrated the World Economy, the less possible it is for individual countries to insulate themselves against disturbances originating abroad. Failure to establish international agreement on national employment policies is therefore bound to cause—and, in the author's opinion, to justify!—economic isolationism. If we are not to forgo all the benefits of international trade, the best we can do is to establish regional unions of countries pursuing identical policies, and to make them as large and self-sufficient as possible. Within these areas there will be "planned economic progress". The countries participating in such schemes would "secure advantages without exploitation, which would not accrue under perfect competition for uncertain markets, and thus offset losses, if any, originating from the limitation in the international division of labour." Dr. Balogh, it is true, bases his case against multilateralism on the familiar assumptions about the inherent instability of the American economy, the inevitability of recurrent crises of the 1929 type, etc., but it does not really depend on them. It is quite possible to reject this hypothesis as phantastic, and yet to accept his conclusion on the simple ground that a country which wishes to conduct its wage and price policy without regard to the rest of the world, will sooner or later be forced to restrict all forms of commercial intercourse with the outside world. This applies particularly to capital movements. "The control of capital movements is not possible without control of all transactions with foreign countries. The aim of the control could easily be frustrated by manipulating import and export prices as well as conditions of credit at which sales and purchases take place." (174). Dr. Balogh is optimistic about the chances of inducing many countries to join such a scheme. Particularly "Small and poor countries are likely to lose most from a restoration of the uncontrolled action of international market forces. The highly imperfect character of the international markets for manufactured goods, the high risk and the automatic acquisition of selling power in the lands of large economic areas will tend to stabilise their existent inferiority." (179). However, he finds it necessary to warn them "against the blandishments of foreign loans" and thus somehow weakens his case.

The sixth paper is an interesting study by Dr. Mandelbaum of the German economy in 1936–8, intended as a case study in full employment. It is somewhat surprising that the author should regard his object as a "major capitalist country".

Scattered all over the book, there are frequent references to "social priorities" as ultimate determinants of economic policy, as though this term had an objective meaning. In the concluding section which deals with "wider implications" we learn that "social priorities will remain a battleground of political interests." (208) The contradiction seems to have gone unnoticed. "The principle of social priorities," Mr. Kalecki cheerfully assures us, "will in any case eliminate such projects as building five bridges over one stretch of the river for the mere purpose of providing employment." (50). Is it very difficult to imagine conditions in which this is precisely what *will* happen if, say, the five bridges are situated in five different constituencies, and the support of the local bosses in all of them is required to work some political miracle?

As Professor Haberler recently pointed out, it is not a peculiarity of soldiers to "fight the last war". We may at least doubt whether economists preparing to fight the vicissitudes of 1950 with methods based on the experiences of 1929 and very little else, are really in a much better position.

L. M. LACHMANN.

[*The following Reviews and Shorter Notices had been set up in type before it became known that it would be possible to revert to the larger type used for those which precede.*—EDITOR.]

The Mining Industries, 1899–1939: A Study of Output, Employment and Productivity. By Harold Barger and Sam H. Schurr. National Bureau of Economic Research, New York. 1944. xxii, 452 pp. $3.00.

Dr. Harold Barger in collaboration with Mr. Schurr has supplied us with a most welcome study of the American mining industry. It has been prepared as a link in a series of connected monographs on the progress of output, employment and efficiency in various branches of the national economy of U.S.A. This ambitious enterprise was started before the war under the ægis of the National Bureau of Economic Research and was financially assisted by the Maurice and Laura Falk Foundation of Pittsburgh with a view of assessing the change in the productivity of labour since the turn of the century. The studies finished so far cover the output of manufacturing industries 1899—1937, the employment in the same industries in relation to the volume of production (both prepared by Mr. Solomon Fabricant and published in 1940 and 1942, respectively), and the output, employment and productivity in American agriculture 1899—1939 prepared by Dr. Barger and Mr. Landsberg (1942). *The Mining Industries* is the fourth completed segment of the research.

The architecture of the volume on mining is as simple as it is effective. In a painstaking way the authors proceed to obtain two sets of time-series for a large number of minerals pertaining to output and employment, the latter presented in a tripartite division, viz., number of men; man-days; man-hours. The second step is the construction of respective index numbers for each industry concerned, and finally, the third step is the relating of the corresponding output and employment indices in order to arrive at the productivity indices for each extractive industry and group of such industries. The main such groups are iron ores, copper ores, other metals (such as gold, silver, lead, zinc, manganese, tungsten, etc., but not placer mining), anthracite, bituminous coal, oil and gas wells, stone-quarrying and other non-metal mining. Notwithstanding the large number of minerals covered the presentation is so lucid and problems are discussed so vividly that almost each page contains arresting results.

The chief factual difficulty with which one has to contend in American mining industry seems to be a somewhat unsatisfactory supply of data pertaining to employment. Unlike in the manufacturing industries the censuses of mining are taken only rarely and far between. With the exception of the Census year of 1902 most of the employment data are derived from the accident statistics computed by the U.S. Bureau of Mines whose coverage and accuracy do not compare favourably with the output figures. Moreover, most of the figures of employment are active-period averages, i.e., over the active period of the year only and not full-year averages. The latter method is used by the Census statisticians though they make some further unusual adjustments, viz.: where a mine has been working less than the postulated 300 days in a year the number of wage earners was proportionately reduced. Thus if all mines worked 240 days employing 100,000 wage-earners, the entry would be only 80,000, i.e., 100,000 x 240/300. The disentangling of such entries presented some serious difficulties which are described in the Appendices, pp. 273 and 321.

Looking at the American economy one is struck by the combination of two factors: the relative smallness of the whole extractive industry and the rate at which the industry was expanding *pari passu* with its productivity. The latter had as its background the continually falling accessibility and availability of the minerals. This race between the progress of technique and the forces of depletion of the natural deposits is truly fascinating. In recent years scarcely 2 per cent. of the American working population was employed in mines, quarries or oil wells and contributed only some 2 per cent. to the National Income. In value terms the fuel group (coal, anthracite, oil and gas) contributed in 1937 nearly three-quarters of the total, the metallic ores were responsible for the lion's share of the remainder. Whereas one in five occupied Americans is engaged in manufacturing, and one in six in agriculture, mines and quarries employ only one in fifty. At the same time, however, while in the last forty years agriculture has lost a good deal of its relative importance, its share having fallen from about 40 per cent. of all employed to under 20 per cent., the extractive industry has been holding its own. Taking physical output in 1899 as 100 the mining index rose to 366 in 1939, manufactures to 374, while agriculture stood at 159. This rise, however, was by no means uniformly distributed in time and it was also largely due to the meteoric expansion of oil wells. Of the three sub-divisions of mining (metal ores, fuels, non-metals) the index of fuels shows the greatest rise and metals the smallest. The latter also show the largest fluctuations in response to business activity. The index ranges from 76 in 1932 (1899 = 100) to 264 in 1916. The extremely low levels of the metal index in 1921 and 1932 are particularly noticeable. The oscillations are largely caused by the high proportion of scrap used in years of depression. Thus in 1932 iron ore output was one-tenth of that in 1929! and less than half of what it was in 1899.

Up to 1929 the mining index for the industry as a whole was rising much faster than population (cf. Chart 2), with the exception of metal ores which, while fluctuating greatly, do not exceed the rate of population increase. Since 1929, however, the old rate of expansion is not maintained. Hence the warning from the authors: "Indeed, it would be accurate to conclude that mining as a whole has reached a declining stage of its development were it not for the sensational growth of petroleum and natural gas production. It is the expansion in the output of these two commodities which has largely compensated for the retardation, or more than outweighed the actual decline, of the older forms of mineral extraction" (p. 18). The rise in output of oil continued all the time from 57 million barrels in 1899 to 1,265 million barrels in 1939. From an ingenious cumulative Chart (p. 198) of reserves and production it is easy to see that, had for some reason new discoveries stopped suddenly in 1930, there would have been no oil left in the wells after 1942. The most astounding increase is that of the production of sulphur which since 1899 shot up 500-fold.

For the falling importance of iron ore the authors have three explanations showing the retarding factors: (1) the substitution of steel in place of cast iron; (2) the economies in the use of raw materials; (3) the substitution of scrap steel and pig iron in the making of steel. We could also add the rising importance of alloy-steel. In this connection it is interesting to notice that the importance of all scrap is increasing with the accumulation of its stock in relation to current production. Unlike fuels, most other minerals remain intact for a long time after their extraction from the mine (cf. Chart 13).

To the English readers the most interesting section will probably be that dealing with fuels, including oil, gas and coal. In the British economy solid fuel is by far the most important mineral both in value terms and as an employer of labour. The growth of fuel in the U.S.A. economy is largely due to the phenomenal rise in the importance of oil and gas. In 1937 more than half the value of all minerals was contributed by crude oil amounting to $1,513 million, less than one-third, *viz.*: $831 million came from bituminous coal, anthracite was worth $198 million, natural gas $123 million, and natural gasoline $97 million. Though in value terms oil is much more important than bituminous coal, the latter yields as much energy (BTU's) as oil and natural gas combined. Since the beginning of the century (1902) U.S.A. petroleum production rose 14-fold and natural gas 10-fold, while the production of anthracite was lower in 1939 than it was in 1899 largely as a result of displacement by the oil-burner. The total of energy supplied from fuel rose threefold during the period. The consumption of bituminous coal did not show any fall until 1917, though there was a rise since 1938. Mining reached its largest share in the total of labour by occupations in the first quarter of the century—it was then 3 per cent., since when it has declined to the level of 1890, i.e., to some 2.2 per cent. Unfortunately statistics of workers by occupations contain the well-known drawback in that they include always a proportion of unemployed labour.

In order to show the American coal mining in perspective we have tabulated below a few salient features throwing in relief conditions in U.S.A. and Great Britain. Of the differences the most striking are those in productivity and in the relative importance of the solid fuel industries.

Changes in employment and productivity in the two countries are very significant. Whereas in U.S.A. employment in solid fuel mining fell by some 15 per cent. between 1902 and 1939, in Great Britain it fell only by 1½ per cent. In the same period productivity (tonnage raised per man) rose by some 50 per cent. in U.S.A. (63 per cent. in bituminous coal and 28 per cent. in anthracite), whereas in Great Britain it rose only by 7½ per cent.

SOME COAL COMPARISONS BETWEEN U.S.A. AND GREAT BRITAIN

	GREAT BRITAIN in 1935		U.S.A. In 1940	
	in thous.	Per cent.	in thous.	Per cent.
1. No. of persons gainfully occupied in the country	21,000	100.0	52,150	100.0
2. Total employment in all mining and quarrying	860	4.5	913	2.2
3. No. of coalminers	764	4.1	527	1.3
4. Per cent. of coalminers amongst all miners	84 per cent.		58 per cent.	
5. Total output of coal and anthracite	222 million tons		500 million tons	
6. Output of coal per miner per annum ..	290 tons		949 tons	
			(In 1938)	
7. Price of coal per ton at pit in 1938	16s. 8d.		$1.87	

The chapter on employment, in spite of the plethora of somewhat irrelevant time-series, is not presented with the same elegance as the information concerning production. This leaves its mark on the subsequent derived concepts. Employment data, occupational data, "300-day-per-year" workers are all mixed up, thus obscuring the situation. Moreover, no distinction is made between the different types of labour. For all this we cannot, however, blame the authors too much; they had to struggle with unsatisfactory statistical material. From the point of view of the reader, all the more regrettable is the omission of the actual time-series in the main text and no amount of elaboration of indices can reduce this inconvenience. What one can gather from the somewhat nebulous concept of "300-day workers" is that their *total* number in U.S.A. mining has scarcely changed between 1902 and 1939 though the distribution of the miners between the various component groups has altered. The chief gainers are oil and gas. It is, however, unfortunate that Table 9 seems to compare unlike entries for 1902 and for 1939. The 1902 employment data appear to be based on the number of the "300-day workers", while the 1939 data are those of total engaged as revealed by the Census of Mineral Industries. In 1939 the totals of the two series differed by some 40 per cent.!

Changes in productivity of labour are obtained by comparing the indices of output and employment. This is done in Chapter 4. Table 11 which bears some traces of haste does not entirely agree with the component tables 1 and 8, e.g., "Metal Mining Total" calculated for 1939 from Table 1 should be 188 and not 183; Fuel Total employment in 1939 is stated as not available while it is given in Table 8; the same applies to oil and gas. We could quote several examples of such disagreements between Table 8 and Table 11, the largest of which is in the case of stone quarrying. In spite of the above shortcomings, however, the results are extremely valuable and give us a good reason for gratitude to the authors. We are shown the great differences in the rate of growth of the productivity per man-hour. The smallest rise occurred in the "old" industries such as anthracite where since 1902 the index rose to 172 (1902 = 100); the largest was in phosphate rock (758), in oil and natural gas (483) and in iron ore (446). On the whole, even excluding oil and gas and allowing for the "300 man-days", the doubling in the mining productivity of U.S.A. in the course of some 40 years is very striking. In the same period *total* mining output and the output per man-day nearly trebled.

The trade cycle specialist will find in the book results which certainly will be worth his while investigating whenever he has to make decisions about his sets of assumptions as to the behaviour of productivity of labour in the mining industry. Taking the industry as a *whole* it seems that one can detect a slight tendency for the productivity to fall in times of depression. This does not apply uniformly to all industries (e.g., copper mining where the lower grade ores are probably abandoned first). In certain other mining industries, e.g., iron ore in times of depression, the open cast works use a larger proportion of labour on removing the overburden, which is not showing itself immediately in any rise of productivity. In many cases where open-cast and deep mines operate simultaneously the more productive surface mines may be more responsive to falling demands. Non-selective mining operating low grade ores on a large scale is, on the whole, more susceptible to the trade cycle. Table 12 comparing progress in mining with other branches of U.S.A. economy is so interesting that we take the liberty of reproducing it *in extenso*:

OUTPUT, EMPLOYMENT AND OUTPUT PER WORKER IN MINING, MANUFACTURING AND AGRICULTURE

	Output	Employment	Output per Worker
Mining, including oil and gas, 1939 (1902 = 100)	283	101	280
Mining, excluding oil and gas, 1939 (1902 = 100)	141	79	178
Manufacturing, 1939 (1902 = 100)	290	149	194
Agriculture, 1937 (1902 = 100)	145	89	164

Finally a word of praise is due for the excellent treatment of the chapters on the technology of mining in which engineering knowledge on subjects such as the transition from selective to non-selective mining, the economics of costs of beneficiating ores and economies of large scale output is judiciously blended with good economic analysis. The picture that emerges from this study is, on the whole, an optimistic one: it seems that in the struggle against the forces of inevitable depletion and the increasingly more difficult conditions of mining the technological progress was so far more than sufficient to counter the sinister forces of scarcity.

A. M. DE NEUMAN.

Quantitative Trade Controls. Economic, Financial and Transit Department of the League of Nations. Allen & Unwin. 1943. 45 pp. 2s. 6d.

There is already an extensive literature dealing with quantitative trade controls, but there was room for a concise review of their main features, objects and results, such as is given in this report. The subject is treated both theoretically and descriptively, and includes an admirable analysis of the reasons for the retention of such controls in the pre-war period. Chapter IV explains lucidly the differences in the operation of tariffs and quotas. The exposition loses nothing in cogency and gains much from the point of view of the general reader by the omission of diagrams and the avoidance of technical language. Any reader can gain from this chapter a clear idea of the functioning and of the restrictive effects of this method of checking trade between members of different nations.

Chapter VII, which sets forth the conclusions emerging from previous chapters, is however disappointing, and may give rise to misunderstandings. The main conclusions are that quotas introduce rigidities " which undermine the functioning of both the price mechanism at home and the system of multilateral settlements "; that they are ill-adapted to a fully-planned and socialised economy, being " essentially a method of controlling the import activities of private firms "; that, on the other hand, if extensively used they are inconsistent with the maintenance of a domestic economy based on free enterprise, because they inevitably induce further controls and tend to give place to public or semi-public monopolies; and that the widespread adoption of this method of controlling trade after the war would indirectly, if not directly, prevent the attainment of the economic objectives laid down in the Atlantic Charter. These conclusions may give the unwary reader the impression that any attempt at quantitative control of trade, even during the transitional post-war period, and even if limited to foodstuffs and raw materials, would be undesirable. This would be at variance with the recommendations contained in other League reports, and on general grounds would be an unwarranted conclusion. This misunderstanding (as it seems to me) arises from the fact, which is not sufficiently emphasised, that the condemnation refers only to national quotas, unilaterally imposed, and not to international quotas, designed to secure, in the short run, the distribution of scarce goods in accordance with need, and to prevent, in the long run, wide fluctuations in production, prices and prosperity.

VERA ANSTEY.

Commercial Policy in the Inter-war Period: International Proposals and National Policies. League of Nations, " Economic and Financial Series ". II, A 6. 1942. 164 pp. 7s. 6d.

This volume constitutes part of a general programme of studies designed to assist in the formulation of a sound and realistic scheme of post-war economic reconstruction. The wise policy has been adopted of publishing, as soon as possible, the results of the various enquiries which have been undertaken. This report fully maintains the high standard which we have learnt to expect from those responsible for League publications on economic topics. Its main object is to compare the commercial policies pursued by States in the inter-war period with the recommendations made by international conferences and committees, official and non-official; to consider the main reasons for the discrepancy between the two, and, in general, for the results of the recommendations made; and to draw conclusions which may prove valuable for the future.

Part I, which surveys the main facts as to the trend of commercial policies and the findings of the various international bodies, leads to the sad but undeniable paradox that although " the international conferences unanimously recommended, and the great majority of Governments repeatedly proclaimed their intention to pursue policies designed to promote ' freer and more equal trade ', yet never before in history were trade barriers raised so rapidly or discrimination so generally practised ". Minor successes were achieved from time to time in specific, limited directions, but on the whole—despite the great and sustained efforts of those concerned with the formulation of international opinion and policies—the net result was a spectacular victory for national economics and autarky. This is true even of the various regional agreements specifically designed to provide a bridge between the existing bilateralism and the desired multilateralism. These regional pacts, of which the Ottawa Agreements formed an outstanding example, led to a diversion rather than to an expansion of trade, increased commercial discrimination, and tended to be used as instruments of commercial warfare.

Why did this occur? Were the recommendations of the conferences misdirected? Or was it merely that counteracting forces proved stronger and frustrated all efforts to extend freer trade?

This report concludes that the recommendations of the conferences were misdirected in the sense that they failed to put first things first, and hence argued on false assumptions. The two main false assumptions were that international commercial policy can be formulated irrespective of the policies pursued in other economic spheres; and that it suffices to demonstrate the long-run benefits of a general reduction in trade restrictions to induce governments "to draw the necessary consequences". These assumptions ignored the fundamental contemporary importance of the depression phase of the trade cycle, and the great changes which had occurred since 1914 in economic structure and conditions. It was useless to appeal for lower tariffs and the removal of the other and more serious trade restrictions so long as world-wide depression and unemployment prevailed or were feared, and so long as national price and cost structures had not been brought into equilibrium with each other. The lessons for the future are that planning in one sphere must be co-ordinated with general economic planning, and that such planning must be realistic and adaptable. The report includes various useful proposals with regard to the procedure which might be adopted by international conferences if sound and substantial results are to be achieved.

VERA ANSTEY.

The Price of Social Security. By GERTRUDE WILLIAMS. Kegan Paul, Trench, Trubner. 1944. viii+199 pp. 12s. 6d.

Report of the Social Security Committee. Published by Authority, Union of South Africa. 1944. 76 pp.

Report No. 2 of the Social and Economic Planning Council entitled Social Security, Social Services and the National Income. Published by Authority, Union of South Africa. 1944. 46 pp. Together with the above Report. 6s.

Report No. 3 of the Social and Economic Planning Council entitled Aspects of Public Service Organisation and Employment. Published by Authority, Union of South Africa. 1944. 8 pp. 1s.

The contrast between the obvious importance of mobility in productive resources and the inadequacy of up to-date literature on the subject has been a patent anomaly in recent economics. If proof were required, Mrs. Williams' bibliography supplies it. Not a single factual survey directly concerned with this vital topic! Not one treatise devoted directly to the logic and principles of mobility! And many of the references she has there noted already out-of-date! So at first blow Mrs. Williams' excellent introduction is right welcome. It gives us the foundation of a practical theory we so long have lacked.

The argument starts by demonstrating the need for mobility of labour, and describing the hindrances to it in modern conditions. The effects of wars, growing monopoly in bargaining, unemployment benefit as an unqualified right, housing difficulties, changing social ideals, and the check to population are realistically and sincerely examined. And the position is honestly faced that the efforts so far made to overcome these difficulties have failed to stem the growth of rigidity. So we are left in the dilemma: if we can find no new adequate incentive to movement, unemployment insurance encourages economic stagnation.

The author then refers to other experiences for a way of escape. From our war experience she concludes that only State direction can fill the bill when speed is essential, as it certainly will be in post-war conditions. For such direction war management supplies many hints. And further chapters show we can learn from German and Russian practice. Even from our enemies we can appreciate the need for a more sincere recognition of society's overriding claims, though we must contrive to express them democratically. And from the Soviet Union we can chew and digest an even more salutary realism devolving from our duty to society: choice of occupation by all means, as far as possible, but finally, if work which society needs is refused, even if of "lower status", then, if the worker chooses to stay idle, no rations, no pay! But as against the stringent side, examples from Russia show that there are available also positive incentives to imagination and elasticity in movement, which can be and have been fostered there.

In her final chapter, then, Mrs. Williams is able to review broadly her initial problem, that social security weakens the incentive to search for paid employment. The remedies advocated are fuller knowledge of openings and guidance, compulsory notification of vacant jobs, training or direction to a new job after a definite period of unemployment, enforced by prosecution if necessary, and finally the need for an adequate advisory economic staff to foresee future trends in industry so that labour may be properly directed as between the different industries.

This book is doubly welcome because it adds to its timeliness three qualities specially precious to-day on this subject. It is entirely honest. It continually reveals an eminently sensible grasp and measure of the psychology involved. And while it does justice to the need for central control, it also insists on the equal need for its democratic organisation, and considers how this may be effected. Space prevents adequate illustration. But a mere male pays tribute to the kind of sound sense and concrete grasp of ordinary human reactions we admired in *Our Towns*. We appreciate, for instance, what is said on the attitude of full- to part-time workers; on the difficulties of compulsory training (a matter on which the Carnegie Trust's excellent *Disinherited Youth* might be compared); on the difficulty workers have in rising above thinking only in terms of the pay envelope; on our "Stay put" traditions and other conventions of this type. And there is brave honesty in the insistence that we must be willing to move, and even move down the ladder, if secure jobs are to be available for the maximum number. Of course, if we did feel the social service were adequate reward, all would be well. But the author is too candid to dally with visions of radical and sudden conversions, preferring to assess even Russian example with a cool if friendly detachment.

Much remains to do in this field. Especially we want more specialised monographs on such as bringing work to workers, trading estates, the relations between mechanical skill and mobility, or the loss through leaving districts derelict. But we must thank Mrs. Williams for an excellent start.

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The three South African Reports are not limited to labour mobility · they cover practically the whole sphere of the social services. Here, in fact, we have the Union's Beveridge Report. To the general reader, seeing how the Beveridge ferment works there is of most interest. To the specialist these reports will be essential as source books.

The Report of the Social Security Committee runs on lines generally familiar here. But the fundamental differences between British and South African conditions and their repercussions are to us of special interest. Thus there are the two major divisions; that into white, coloured and Asiatic, and native beneficiaries; and that between city, town, and rural dwellers. The large discrepancies between the contributions and benefits of these two sets of orders cannot fail to impress us living in these homogeneous, urbanised islands. (For instance, a British reader, used to our greater mobility, is apt to wonder how you live, when you retire to a city, on your old age pension subscribed and paid at the proposed rural rates.) But the firm statement of fact compels us to see that rapid and fundamental departure from the existing social foundations would do more harm than good. We are sensibly impressed by the vast scope for non-European relief. But improvement must build on the existing social framework and aspirations, especially for the native communities. The committee does, however, plan for a steady up-grading. They remain very conscious of the prior need for a huge expansion of the national income if the desired improvement is to spread over the whole population. The existence of this production problem they heavily underline. And while unemployment there is lower than we have had to suffer, they insist that there is the greater need to preserve a margin of incentive between benefit and wage rates—a difficult problem where some rates are so low, and vary so for different racial groups. Family allowances for all seem to provide a neat solution. But here again, they insist, a greater national income is a *sine qua non.*

Report No. 2 underlines and drives home the first Report, especially as to needs, costs, and the way to meet them. It is no criticism of the Beveridge Report to say that in this South African survey the economic atmosphere is more bracing. For here the problem of cost, alternative cost, is posed as the crux of the whole situation. Thus the Council faces the need to prepare for the expected check to the gold industry; and definite priorities in social services are suggested relative to productive possibilities and the ways in which they in turn can be geared to social schemes. Other proposals, suggestive for us, are central depots for *real* subsidies to consumers, to cut out excessive retail margins; and school hostels to assist the proposed rapid improvement in education (not only technical) which the Council insists is necessary for economic and social survival.

Finally, Report No. 3 outlines the immediate *administrative* arrangements necessary if the plan is to be developed as an integral whole. The measures to avoid friction between departments are sound applications of the *principles* we at least sketched in the Haldane Report. And wise provision is also suggested for training adequate civil service personnel and evolving the necessary information and technique.

A. L. Macfie.

Design of Accounts. By F. S. Bray and H. B. Sheasby. Oxford University Press. 1944. 208 pp. 12s. 6d.

The publication of this book deserves a cordial reception, since it implies a clear recognition of the obligation of members of a profession to promote the most effective use of its techniques and methods. Moreover, the practice of the Society of Incorporated Accountants and Auditors in promoting the preparation of such books, while withholding the stamp of their authority from the particular views expressed by the authors, appears to have much to commend it; otherwise, it is difficult to see how tentative statements of principle can be laid on the table for discussion, an essential preliminary to the systematic building-up of a " body of knowledge, available for the whole profession, present and future "—to quote from the preface contributed by Mr. Whitty, the President of the Society.

There are two useful chapters on the principles of design and the interpretation of accounts, the second devoting considerable attention to the use of ratios as a means of escaping the accountant's customary over-preoccupation with purely monetary costs. There are also appendices containing reprints of the recommendations on accounts and accounting principles issued respectively by a committee of the Society and the council of the Institute—information which is otherwise only available at present by delving into back numbers of professional periodicals. But by far the greater part of the book consists of specimen forms for published accounts and for accounts useful for internal management purposes.

Now that the main lines of recent developments in the lay-out of published accounts are becoming fairly well-known, little need be said on that score, save that it is good to see the authors coming down definitely in favour of disclosing sales figures in profit and loss accounts as well as separate figures for production, selling and distribution, and administration costs—a disclosure they regard as necessary if accounts are to be adequate, which is " but one return for the privilege of limited liability ". As to the forms for internal management use, however, it is evident that the authors are unaware of much that industrial accountants have achieved in the last fifteen years in the technique of cost presentation. For it appears that they are still influenced far too much by the ledger-book obsession of presenting information in account form, with debit and credit sides to it; and perhaps the most noticeable shortcoming is the complete absence of any reference to, or apparent realisation of the effect of, standard costing—a technique which inevitably alters drastically the manner of presenting information, because its very possibility alters management's whole conception of the information it needs for effective control of a business.

For all that, however, the book is greatly to be welcomed. To quote Mr. Whitty again, it "marks the beginning, and not the end, of a process of discussion".

BASIL SMALLPEICE.

Commodity Control: A Study of Primary Products. By P. LAMARTINE YATES. Jonathan Cape. 1943. 248 pp. 15s.

Some years ago a group of members of the Fabian Society undertook a study of a number of primary products. Owing to the interruption of the war the work was not able to be completed as a collective undertaking, and it was left to Mr. Lamartine Yates to produce the present volume which contains separate studies of the following eight commodities: wheat, sugar, coffee, cotton, rubber, tin, copper, and mineral oil. Mr. Yates accepts sole responsibility for the general conclusions.

In so short a space it is not possible to deal very thoroughly with all the individual products and the different studies vary in value. Each study opens with an account of the product in question and of its sources of supply and conditions of demand. Then follows an analysis of price movements and a brief survey of such control schemes as have been attempted in the past. Finally, suggestions are put forward for some more permanent type of organisation in the post-war world.

In his conclusions Mr. Yates divides the primary products into two classes—those for which demand is likely to continue to expand after the war, such as rubber and mineral oil, and those for which demand is likely to be static or declining, such as wheat, sugar, coffee and cotton. Incidentally, the fact that natural rubber is placed in the former class implies a "view" as to the prospects of synthetic rubber production which by no means everybody will share with him. For the expanding class he recommends buffer-stock control, and for the contracting class a quota system associated wherever possible also with the establishment of buffer-stocks. Mr. Yates discusses both these methods of control at some length and shows that he is alive to some of their possible drawbacks and dangers as well as to their advantages, though it is doubtful whether he attaches sufficient weight to the practical difficulties involved. The author, and the group with which he is associated, have a natural bias in favour of some form of organised international control, with a strong element of consumer representation. It is for those who disagree with this approach to the problem to show that either *laissez faire*, or producers' agreements on the old lines, will call for that efficient and continuous development of the output and marketing of primary products which the world will need for its economic wellbeing in the years to come.

Taken as a whole this book can be recommended as a useful introduction to the study of the thorny and intractable problems which will have to be faced in the sphere of foodstuffs and raw materials after the war. It is to be regretted, however, that some of the most interesting commodities, such as hard fibres and vegetable oils, have not been able to be included in the survey.

C. W. GUILLEBAUD.

The Development of Public Services in Western Europe, 1660–1930. By SIR ERNEST BARKER. Oxford University Press. 1944. viii + 93 pp. 5s.

This book, a reprint of part of the author's contribution to a collective work published several years before the present war, compresses in relatively few pages a very considerable amount of interesting information. Although historical in character, it is by no means without significance for the solution of to day's and to-morrow's problems, for, as the author aptly remarks: "It is the permanent past, not the fleeting present, which will be the basis of the permanent future". The spirit of impartiality and scholarly detachment with which it is written gives it a high rank among books of its kind.

The countries chosen for showing the growth and character of public services in Europe between 1660 and 1930 are England, France and Prussia, and the services surveyed on a comparative basis are Administration, Conscription, Taxation, Social Services and Education.

The section on "Administration" is the longest and also the most important. It gives a very clear picture, against a historical background, of the administrative structure of the three states, including the relations between Local and Central Government, and serves thus as a key for the understanding of the special services dealt with in the following sections. Of these, the parts dealing with the military organisation and the educational system are likewise of great interest, while "Taxation" and "Social Services" are treated somewhat summarily. It is in respect of these two subjects that the economist and sociologist will find himself most at variance with Sir Ernest Barker's general dictum that the evolution of the Modern State has resulted "in the freeing of the action of the State from the play of Society". Although this dictum may be justified in a strictly legal sense, it certainly does not apply in a sociological and political sense, for it can hardly be denied that economic and social forces are a strong factor in framing the action of all departments of modern public life. Nowhere is their influence more manifest than in determining the character of the system of public finance and in moulding the organisation of the social services. Apart from this divergence in the general interpretation of the development, there are several other statements which are in discordance with the accepted definitions and teaching of Political Economy and Public Finance. Thus a tax which, like the old French réalité de l'impôt, is laid on things and not on persons, is not, in general, on that ground regarded as an indirect tax. The proposition that taxation should be used not only as a means of raising revenue, but also of effecting a redistribution of income and property, need not have full equality of income or property as its object. Protectionist customs duties, although up to a certain point a source of revenue—most when they fulfil their protectionist object least—cannot in general be said to be " taxes on other nations ", since in the majority of cases they are paid by the domestic consumer of the country imposing the duties.

A few slips occurring mainly in the paragraphs on Prussia might be corrected in a new edition. Frederick the Great died in 1786; the Allgemeine Preussische Landrecht is usually quoted as dating from 1794; the minister to whom the high standard of education in Prussia after 1815 was largely due was von Altenstein (not Allenstein). The German railways before 1919—with the exception of those of Alsace-Lorraine—were in the ownership of the larger Federal States, and income derived from them was one of the main sources of revenue of these States, mainly Prussia, and not of the German Empire. Social Insurance in Germany was first extended to clerical workers by the Empire in 1913, not by the Weimar Republic after 1918.

As a whole, the book provides an excellent introduction into an important sector of European history as manifested in its three outstanding national varieties, and the general lesson which the author emphasises in the concluding passage is impressively conveyed throughout, namely that all States owe a debt to one another. " There has been rivalry in methods, but it has not been unfriendly; one country has studied, adopted, and tried to improve the methods of another; and all have combined, however unconsciously, to promote the growth of a common European standard of administration and public service."

CHARLOTTE LEUBUSCHER.

The Journey to Work: Its Significance for Industrial and Community Life. By KATE K. LIEPMANN. Kegan Paul. 1944. 199 pp. 15s.

Dr. Kate Liepmann has written a useful monograph (the first of its kind to be published in this country) on the problems arising from the daily movement of the employed population between their homes and their work. While the largest number live within convenient walking or cycling distance of their jobs, many also go by train and/or, by 'bus and tram, and the journey to work is often a lengthy and ardous as well as an expensive matter. The latter part of the book is devoted to an analysis and summary of the statistical material available for this and other countries, the principal fact to emerge from which is the extreme paucity of our knowledge of the relevant facts and the need for further information on a matter which is clearly of very real importance.

While recognising the advantages of the journey to work from the point of view of increasing the mobility of labour and giving the worker access to alternative labour markets, Dr. Liepmann also stresses its negative aspects—additional fatigue, more liability to sickness, extra expense, as well as important social disabilities. She puts forward a number of useful suggestions for mitigating some of these evils. Her principal, and in many ways most interesting proposal raises the whole question of the future of town-planning in this country. She deprecates, by no means solely on the special ground of its bearing on the journey to work, the policy we have adopted since the last war of open development all round our towns and cities, with a twelve houses-per-acre limit as the general standard of density. In place of the garden city idea, which has found so many adherents in this country, she would substitute where possible the compact satellite town, divided from the main town and place of work by a green belt, but itself housing a comparatively large number of persons per acre. She points out rightly that there is not enough land in England for towns to go on spreading over the countryside as they did during the inter-war period, while costs of travelling to work, of delivery and of services of all kinds, increase with the size of the area over which an urban population extends. In a small compact town a civic and community sense can develop in a way that is not possible in these monstrous suburban aggregations which have no independent existence of their own.

Dr. Liepmann's book is to be commended to those who are interested in the future of housing after the war, as well as to those who are more directly concerned with the many-sided problems to which the daily journey to work gives rise.

C. W. GUILLEBAUD.

Controlled Trade. By N. MOMTCHILOFF. N.I.E.S.R. Cambridge University Press. 1944. 85 pp. 6s.

It is a depressing fact that it was left to Dr. Schacht to demonstrate to the pre-war world that the object of exporting is to import and that consequently the less a country exports in payment for its imports the better off it is. From Mr. Momtchiloff's account of the experience of south-eastern European countries in the last 10 years it would appear that this somewhat neglected truth is by now well appreciated.

The German policy during this period was to fix a rate of exchange at levels which overvalued the mark, encouraged imports into Germany and led to the accumulation of considerable clearing debts by Germany to the south-eastern countries, which received a dwindling amount of high-priced German exports. In consequence the various economic controls in the south-eastern countries have had to be stood on their heads. In place of obstacles to importation, import subsidies have made their appearance. In place of the scramble to export there are systems for lessening and restricting exports. Mr. Momtchiloff has many interesting and useful comments on these developments, and suggests that in the post-war period, when controls on distribution may have to be retained, there should be some machinery for regional co-ordination where the results of the experience of the last ten years can be pooled and assistance from the richer countries organised. There are obvious difficulties about limiting the area of co-ordination to a group of countries in south-eastern Europe, but this solution is no doubt preferable to leaving those countries " to stew in their own sovereignty ", to use Mr. Momtchiloff's picturesque phrase.

Mr. Momtchiloff's book, though marred by imperfection of English in some places, is a good guide in difficult territory.

A. S. J. BASTER.

SHORTER NOTICES

International Aspects of Wartime Monetary Experience. By RICHARD A. LESTER. (Essays in International Finance, No. 3.) Princeton University. 1944. 22 pp.

By his *Monetary Experiments* (1939), Professor Lester placed us in his debt for the results of some enterprising research into the vagaries of currency issues in two critical periods. He has now provided, as it were by way of postscript, a brief but illuminating discussion of some comparable wartime phenomena—the issue of military currencies, the problem of exchange rates in occupied territories, and the application of Gresham's law to paper monies. He has a keen eye to the theoretical moral, and although his pamphlet is designedly no more than a sketch, it opens up a little-tilled field, which deserves, and will ,we hope, receive more intensive cultivation as further facts become available.

Dismissal Compensation. By EVERETT D. HAWKINS. Princeton University Press. 1940. viii + 390 pp. $4.

This is a competent and thorough summary of the facts and problems of dismissal compensation, in the United States and other countries. Dismissal compensation is not a substitute for a system of unemployment insurance; though often of wider scope, its main purpose is to put the burden of unemployment on the individual company where permanent dismissal is due to mergers or technological change. The author examines plans adopted voluntarily by a number of American companies, schemes embodied in collective agreements with trade unions, and legislative measures.

The Legacy of Nazism. The Economic and Social Consequences of Totalitarianism. By FRANK MUNK. The MacMillan Company. New York. 1943. 288 pp. $2.50.

An ambitious and fascinating title, but a disappointing book. The author never decides what it is he wants to get at and tries everything: what was, is, can be done, will be done, ought to be done, in Germany, occupied Europe, the United States, the whole world—in breathless haste, settling nothing. This is a pity, for the author's stress on the interdependence of economic, political and psychological factors is refreshing, and his many penetrating observations, e.g. on the " politicalisation " of price determination, deserve a better setting.

The History of Rubber Regulation, 1934–1943. Edited by Sir ANDREW MCFADYEAN for the International Rubber Regulation Committee. George Allen and Unwin, Ltd., London. 1944. 239 pp. 10s. 6d.

This is a severely factual account of the work of the International Rubber Regulation Committee during the nine years of its existence. The texts of the different Regulation Agreements and a certain amount of statistical information are given in Appendices. There is very little discussion of the wider significance and effects of regulation.

British Wartime Price Administration, 1939–1934. By JAMES S. EARLEY, L. MARGARET HALL and MARJORIE S. BERGER. Office of Price Administration. 1944. 102 + xviii pp. Mimeographed.

This is a painstaking survey of the purposes, machinery and effects of the control of British prices during the years 1939–43, revised from an earlier version dated January, 1942, by the inclusion of some (but not all) corresponding figures for 1943. Two general studies, on the outlines of the system and on the price indices, introduce chapters on the activities of the Ministry of Supply, Ministry of Food, Board of Trade and Ministry of Fuel and Power, with final notes on Transport, Rent and Wages. Although the bulk of the text, relating mainly to 1940–42, sometimes sounds a little old-fashioned to-day, it does of course cover the most active and formative years of the controls discussed.

The collation of the hundreds of individual facts, derived from scores of official documents —it is fearful to think of the number of Statutory Rules and Orders which the authors must have assimilated—has, so far as a layman may judge, been done with an excellent sense of proportion. Where comment is added it is, generally speaking, relevant and impartial. That the result, valuable as it is, will arouse no enthusiasm in the breast of the ordinary reader is a reflection on the average Englishman's monumental lack of interest in the machinery by which he is governed, and not at all on the enthusiasm or style of the authors. They will be rewarded by the thanks of the students of administration, to whom their compilation will prove a helpful digest of a most intractable mass of material.

Wartime Government in Operation. By W. H. NICHOLLS, Ph.D., and JOHN A. VEIG, Ph.D. The Blakiston Company, Philadelphia. 1944. xiii + 109 pp. $1.50.

The staggering productive effort of the United States in the present war could hardly have been accomplished without work of direction and co-ordination of a very high order. There is no doubt that the government service in Washington during wartime has secured many first-class administrators, and has made good use of some of them, but the mounting volume of criticism since 1941 has left a clear impression that much time was lost, much effort wasted, and many opportunities missed in the organisation of the war economy. Great as the American effort has been it might have been greater still if the economic staff work had been better.

The book gives the stock explanations for this state of affairs; the dog-in-the-manger attitude of Congress, the unhappy administrative expedients of the President and the pressure of special interests. It suggests a Congressional Joint Committee on the conduct of the war based on the model of the Truman and Nolan Committees, a "legislative cabinet", holding weekly conferences with the President, an overhaul of the executive agencies, and a "legislative veto" by Congress on the *plans* of the Executive leaving the latter increased freedom in matters of detail.

The book is useful for those who have not followed developments in this field at close quarters but the subject really needs more fundamental treatment at greater length.

A. S. J. BASTER.

The Labor Force in Wartime America. By CLARENCE D. LONG. Our Economy in War: Occasional Paper 14. March, 1944. National Bureau of Economic Research Inc. New York. 73 pp. 50 c.

In this pamphlet the author traces the extent to which net additions have been made to the labour force in the United States during this war and the last. He compares American wartime experience with that of England and comes to the conclusion that the lower American ratio of gainfully occupied in the productive age groups can be explained by the higher proportion of students and of married women with children in the U.S.A.

General Censuses and Vital Statistics in the Americas. Prepared under the Supervision of IRENE TAEUBER. U.S. Department of Commerce and Bureau of the Census. Government Printing Office. Washington, 1943. ix + 151 pp. 65 c.

This is an annotated bibliography of the vital statistics of all the American independent republics and of dependent territories. A short introduction deals with the historical development of census taking and vital statistics in each territory. Some non-official titles are also given.

Books Received

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Clapham[1] on the Bank of England

By Jacob Viner

Sir John Clapham wrote this history of the Bank of England at the invitation of the Bank, as part of its celebration of its 250th anniversary. This is not, however, one of those ceremonial histories of banks with which we are only too familiar in which the excess of uncritical praise is matched only by the deficiency of significant information. This is a thoroughly scholarly account, heavily packed with relevant detail, of the record of the Bank through rough times and quiet years, in its relations with the Goverment, the money market, its customers, and its shareholders. All the records of the Bank were thrown open to Sir John, and he was left perfectly free to write as he saw fit. While the result is in my opinion somewhat too generous to the Bank, the occasion called for a reasonable display of human kindness, and at no time does the work descend to eulogy, while at some points the comments have a sharp enough sting. Except for a brief " Epilogue ", the history ends with 1914. All comments made in this review on the record of the Bank are likewise without reference to the years since 1914.

Everyone is agreed that the Bank of England has been a remarkable institution, and this history makes it clearer in what respects it has been remarkable. Perhaps the most unusual aspect of the Bank's record has been its longevity. No other bank except the Bank of St. George, Genoa, as far as I know had anything like as long an unbroken history, and an earlier start than most of its rivals is not a sufficient explanation, since it had predecessors and since it has seen many other institutions rise, have their comparatively brief day in the sun, and then vanish—usually with a bang. Except for the Bank of England, it does not seem to be in the nature of banking institutions to last forever, or forever to deserve to last. The nearest rivalry in durability that can be claimed for any lending agency, public or private, that I know anything about, is that of the Amsterdam private banking-house of the Gebroeders Neufville, which collapsed during the international crisis of 1763 after something like two centuries of active operations, and survival seems to have been more difficult for financial institutions in the late eighteenth and the nineteenth than in the seventeenth and early eighteenth centuries. The Bank of England had one or two narrow squeaks, and it had its due share of good luck. But its durability, financial and political, must be attributed mostly to the conservatism of its management and to the moderation, at least since 1689, of English politics.

The continued, and for the most part steadily-growing, prestige of the Bank is also a remarkable feature of its history, especially if it is considered that its resources were never great, its range of activities unspectacular for the most part, its directorate on the whole a very moderately distinguished lot either in the financial field or otherwise, and its facilities for winning public favour by well-designed publicity as to its achievements practically non-existent. Not much contribution to an explanation of the continued prestige of the Bank, as far as I can see, is made by this study of its history. It may be that the public did not like the long succession of critics by whom the Bank has been tormented, for these critics while often able and well-informed were also often partisan and sharp-tongued. It may also be that the longevity of the Bank largely explains its prestige—and the prestige of the Bank its longevity—especially since it operated in a country which has had the gift of endowing even its raw vintages with savour if it poured them from ancient enough decanters, and has had the sense not to smash its old bottles.

Remarkable also was the constitution of the Bank, if it be regarded as primarily a central bank, with major responsibilities for regulating, in the national interest, the national currency and keeping the national money market in good order. The authority for managing the Bank's affairs rested throughout its history with a small inner group of its directors, selected from the ranks of its shareholders. Anyone who wished to could buy shares in the open market, which were almost always a good investment and sometimes a good speculation, and except at the foundation of the Bank apparently no one ever bought the shares, or was asked to buy them, otherwise than on the basis of their financial attractions. The directors, moreover, were further limited to London merchants and merchant bankers, could rise to the highest Bank offices largely by seniority and financial respectability, without need of other more intellectual qualifications, and as directors were not expected to suspend any of their other business activities, even when these were such—as they often were—as to bring into direct conflict their personal interests and the interests and responsibilities of the Bank as central bank. No one, I am sure, would to-day even dream of setting up a new central bank on such a pattern. It is a tribute to the basic integrity and sense of social responsibility of the London businessman, and to the benevolent disposition toward England of a watchful Providence, that throughout its history the Bank escaped all but minor scandals and never suffered from worse than moderately bad management.

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[1] Sir John Clapham, *The Bank of England*, Vol. I, *1694—1797*; Vol. II, *1797—1914*. The Cambridge University Press, Cambridge, 1944. ix, 305 pp.; vii, 460 pp. 48s.

Sir John Clapham's history of this remarkable institution is a notable achievement, a splendid addition to his long record of productive scholarship in the field of economic history. To have accomplished it in the midst of the strains and stresses, physical and psychological, of a beleaguered England makes it all the more noteworthy. If the reviewer, nevertheless, asks for more at some points than the author has given, or asks for a different way of handling the available factual material, this is not intended to suggest that Sir John was under any obligation to write a different kind of a book. It is in the nature of economic historians to write historical narrative, as of economic theorists to expound hypotheses unsupported by historical investigation, and there is no reason to expect the twain to meet. Sir John, in particular, has made clear that he is not fond of the empty economic boxes of the theorists and has not made evident any great urge to fill them with the product of his own historical researches. His detailed account of the history of the Bank, if supplemented in several directions, should, however, make possible a more searching analysis of the rôle of the Bank in relation to the British national economy and to the international gold standard than has hitherto been practicable. It is predominantly with a view to suggesting some of the possibilities in this direction that I later venture to point out some of the aspects of the Bank's history on which we still are left knowing less than it would be good for us to know.

Meanwhile, I must pay tribute to the merits of Sir John's English, which I find admirably suited to his purposes. The book before us is marked throughout by rigorous economy of verbiage, precision of expression, unadorned but none the less graceful simplicity of exposition. The severity of the style is engagingly softened here and there by skilful turns of phrase which reveal a sense of humour kept under strict control. There is a welcome interest in the personalities of the important actors, and a notable capacity to sketch them revealingly in a few deftly-chosen words.

We are told much that was not known before, or not known clearly and authoritatively, about the operations and the fortunes of the Bank, its leading spirits, and the sources of its income. This is especially true with respect to the eighteenth century, and if sometimes what we are given suffices only to whet but not to satisfy our appetites, it is usually the meagreness of the Bank's records and not any shortcoming on Sir John's part which is responsible.

We are told a good deal about the operations of the Bank as, in effect, accountant and paymaster of the National Debt, and we get a glimpse of the hard bargaining which periodically recurred as to the terms on which the Bank should perform this administrative task for the Government. That this routine governmental function should ever have been entrusted to the Bank and, once so entrusted, that it should have been left there and indeed increasingly lodged there, without ever, apparently, giving rise to any demand from any quarter that it be recaptured, seems to me, however, to call for some explanation. In the days of *laissez faire* and civil service, it could no doubt be explained as a reflection of the Government's aversion to administrative burdens untempered by any realisable ambitions for the fruits of patronage. But what about the eighteenth century, when *laissez faire* was perhaps already practice but not yet principle, and when even grossly corrupt patronage was thoroughly respectable in the highest of circles? It would be interesting to know whether any other government, even within the British Empire, has ever made a similar delegation of its routine administrative obligations with respect to its internal debt. For the Bank, the importance of its management of debt-service seems to have been confined to its constituting a substantial, and needed, source of revenue.

On the most important aspect of the Bank's history from the national point of view, its rôle as regulator of the national money market, something, but not very much, is added to what was known before, and little if any revision is called for of the previous accounts with respect either to the general sequence of events or to the appraisal of the quality of the Bank's functioning. It seems clear that as a rule in times of crisis the Bank kept its head and did as much as it knew how to do—which at times was not much—occasionally at heavy cost and with rather serious financial risk to itself. But in quiet times and in times of prosperity it pursued with full vigour the maximisation of its profits, and it is not clear how its pattern of behaviour at such times differed from what it would have been if there had been absolutely no question of its having any special public responsibilities or any obligations not equally shared by all other respectable banking institutions. By no means always, but occasionally, Sir John himself seems to take rather lightly these special responsiblities of the Bank, and to accept its own mediocre standards of proper behaviour. For instance, with reference to an increase in dividend payments by the Bank in the crisis year, 1847, he comments: " A year of heavy discounts always provided funds for good dividends, and the Court is not to be blamed much for proposing them, nor proprietors for accepting " (II, 204). Or again, with reference to 1907: " Reserves might be too small, as everyone had been saying for thirty years and more, but they were handled and thrown into the fight with effect. And if that could be counted on, the British economy in reserve keeping could be defended. There is no call for bigger strategic reserves than will win your battles " (II, 389). True perhaps. But it was only the Bank's battles which these reserves sufficed to win completely. *England's* battles were on such occasions always lost, partially at least.

It is a commonplace that England was during the nineteenth century the efficient manager of the international gold standard, and that the Bank of England was the agency through which this management was applied. It is extraordinary, however, how little systematic study of the workings of the pre-1914 gold standard has ever been engaged in, and how little concrete evidence has been available as to the extent or the quality of its management. Except for the contribution to the prestige and the durability of the standard which England—and the Bank—made by maintaining unquestioned convertibility of " sterling " into gold throughout almost a century and by maintaining during the same period an absolutely free gold market, this history of the Bank reveals little or nothing to confirm the belief that the Bank, or any other English institution, deliberately or otherwise " managed " the gold standard.

Management of the gold standard could have taken a number of forms. By accumulation of excess gold reserves in good times and their release in times of strain, the Bank, or England, could have exerted a stabilising influence on world price levels, counteracting both inflationary and deflationary tendencies as they manifested themselves. But the outstanding feature of English gold reserves during the nineteenth century was their inadequacy not only for this global task but even to preserve convertibility of sterling without frequent recourse to otherwise wholly undesirable deflationary pressure by the Bank on the English national economy. It may be that the need of the Bank for income for dividend-paying purposes was an adequate justification for the Bank's failure to accumulate larger gold reserves from its own capital resources. But there were many ways in which the Government could have made it possible for the Bank to acquire adequate gold reserves without depletion of its revenues. As far as England as a whole was concerned, it managed to operate its part of the international gold standard throughout the nineteenth century on an investment in gold stocks pitiably small in relation to the benefits which would have accrued to it and to the world if there had been less parsimony in this connection. The Banque de France and the Reichsbank seem to have had a better record in this respect.

There can be no doubt that London in the nineteenth century provided a wholly efficient and economical clearing-house for the exchange-business of the world. Until the 1880's, in fact, practically all overseas financial transactions were cleared in London and in terms of sterling. But this was profitable business for the London houses engaged in it, and if London had provided poorer facilities or charged higher commissions other money markets would gladly have replaced it. I know of no evidence as to the extent to which London was " banker " as well as " clearer ". To the extent that sterling balances were serviceable abroad as monetary reserves, London, by granting credit abroad more freely in times of strain and contracting it in times of dangerous expansion, could have exercised a stabilising rôle *vis-à-vis* the international gold standard despite the exiguous character of her gold reserves. But I know of no evidence that London played such a rôle, and as far as available information goes it may even be possible that London typically reduced her credits to abroad when it was most urgent that she should expand them and expanded them when contraction would have been more beneficial. It seems clear that as far as long-term loans were concerned England accentuated the cyclical fluctuations in the level of world activity rather than damped them down, and this for all we know may have been true also of her short-term transactions.

There were also important possibilities in international collaboration between central banks in dealing with local or general financial strains. But the Bank of England never showed any interest in developing connections with other central banks and in systematically planning in advance for collaboration in case of need. Instances of such collaboration were not rare in the nineteenth century, but they were always hastily extemporised at the critical moment, and in so far as the Bank of England was a participant it was as recipient rather than as giver of aid except for at the most two or three occasions.

In this field, the Bank of England was completely unenterprising and unimaginative. It contented itself with looking after convertibility of its notes, and left it to other countries to keep their monetary affairs in order in the same narrow sense. If there developed strains on its gold reserves more severe than it could confidently cope with, it was not too proud to accept help from outside when offered to it, and perhaps even upon occasion to solicit it. But as far as the international aspect of the gold standard was concerned, there is nothing in the record of the Bank as related in the work before us, or as previously known, which gives any support to the claim that the Bank "managed" the gold standard. Sir John quotes a former Bank official, Sir Ernest Harvey, as making even a harsher appraisal than this for the period before 1918: "the Bank was amazingly detached from international affairs; heard from no one; saw no one; only watched the gold and took the necessary steps semi-automatically" (II, 401).[1] The meagre capital resources of the Bank, the absence of legislation which would force the large commercial banks to provide the central bank with the monetary resources necessary for effective control of the money market both in its internal and its international aspects, and the restricted view the Bank took of its responsibilities, may have had as a fortunate by-product that the Bank was not likely to involve England in serious economic difficulties through foolish positive action on its part. But they also prevented the Bank from even approaching full realisation of its possibilities for service to the national economy and to the world economy.

Sir John, in this and other connections, never disregards the major issues in the record of the Bank about which controversy has been waged for over a century, never lapses into unwarranted praise of the Bank or into treatment of its shortcomings as if they were virtues, and occasionally makes adverse criticisms which are not less effective for being made calmly and regretfully. His general procedure, however, with its emphasis on the year-to-year sequence of events and its disemphasis on systematic appraisal, operates on the whole to give a more favourable impression of the quality of the Bank's administration of affairs than would have resulted from a more analytical examination of the record.

Sir John provides us with some useful statistical data, drawn from the books of the Bank, but more of such material would have been welcome, and there will be extensive scope for comparison by later workers in the field of the data he does provide with other statistical series bearing on the internal and international status of

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[1] An earlier Bank officer, John William Birch, once gave a more flattering account of the Bank's policy and procedures in this connection. In view of the scarcity of informative policy statements emanating from the Bank itself, it is worth quoting *in extenso:* "When I entered the Bank I remember in our discussions as to raising or lowering the Bank rate we were always talking about the balance of trade and trade returns. We know now that the balance of trade has very little to do with our international balancing. The great thing which governs us is the enormous transactions on the Stock Exchange, in France, in Germany, and in the United States. These are the operations we have to follow most carefully. We have to observe the course of the foreign exchanges, and they are more carefully watched than they used to be. We have also to follow the movements of gold, and within the Bank we are not simply working *le jour pour le jour*, but we try to look ahead and forecast what is likely to result from the negotiation of public loans, not only in this country but on the Continent. For instance, if a loan is contracted in Germany for the Argentine Republic it is more than probable that if gold is required, it will be taken not from Germany, but from England". *Journal of the Institute of Bankers*, vii (1887), 509–10. Birch was a director of the Bank from 1860 to 1897 and Governor in 1879—1881.

England. The data which he presents with respect to the Bank's loans and discounts in the nineteenth century are given in terms of the revenue they produced for the Bank instead of in the much more significant terms of the fluctuations in their absolute amounts, but this is presumably to be attributed not to undue interest in the effect of the Bank's operations on its own financial fortunes but to the manner in which the Bank kept its accounts. It would be useful to know, with a view to the facilitation of further study, the full extent of the records of the Bank available or convertible into statistical form for the period, say, from 1832 to 1914.

There follow some minor queries or emendations or elaborations on details of Sir John's account of the history of the Bank.

A Court Vote of November, 1696, is referred to which gives depositors " liberty to transfer any Sume from one Account to another *not* under £5 " as probably not starting a policy of permitting inter-depositor transfers by check but only sanctioning and regulating one " that had grown up to meet daily needs during those years ", *i.e.*, 1694 to 1696 (I, 141, italics mine). But there was an earlier vote, in May, 1696, publicly advertised, which extended the same privilege to transfers of " any sum under £5 " as a temporary measure to meet the shortage of means of payment while the recoinage was under way. Is it not as likely that it was the recoinage which first gave rise to the practice as that, as suggested by Clapham, the Bank had " permitted it from the start " ? (I, 142).

Sephardic Jews are referred to, as late as the early eighteenth century, as having " Portuguese " and " Peninsular " connections in the trade in bills of exchange and bullion (I, 127 and I, 134–35). This was no doubt true of the Sephardic Jews in the first half of the seventeenth century, but had not all their ties with Portugal and Spain ended before the end of the century ?

We are given, for the first time, a consecutive account of the interesting " Subscription for the Circulation " established in 1711 and continuing until 1759 (I, 67–72). The Bank each year contracted with the Government to " circulate ", or give to holders cash upon demand for, a specified amount of Exchequer Bills, and each year took subscriptions from the market, of an underwriting character, for corresponding amounts. Of these subscriptions, 10 to 20 per cent. were immediately payable in cash, and the remainder upon demand from the Bank, with forfeiture of the cash already paid in case of failure to meet the call. Interest was paid on the cash subscriptions and a " premium " on the amount uncalled, with the total payments working out at a very generous rate of interest on the actual amount of cash subscribed. Calls for payment were infrequent, and seem to have been made only when the Bank was being subjected to a run. This device gave the Bank a call on the money market in case of need and provided a more-or-less assured basis in advance for " borrowing from the market " to replenish its reserves when necessary. Some additional details of the history of the Subscription which I have run across recently may be of interest.

It seems probable that it is to Charles Davenant that the Bank—or the Government—owed the idea of the Subscription. He proposed in 1710 a scheme for financing the Government through issue of exchequer bills whose circulation was to be assured by a method identical with that adopted in the following year, except for one significant detail. Davenant, as a Tory, was hostile to the Bank, and under his scheme it was the Government and not the Bank which was to take subscriptions for circulating the exchequer bills (*New Dialogues*, Vol. II, 1710, pp. 164 ff. For Davenant's authorship of this tract—as well as of several others of the period credited by Sir John Clapham to others—see A. P. Usher's Introduction to *Two Manuscripts by Charles Davenant*, The Johns Hopkins Press, Baltimore, 1942.)

The Subscription was nominally open to the public, but it was so profitable to subscribers—if no call was made—that it was in practice reserved for Bank insiders. A printed copy of what appears to be a subscription blank for the Subscription of 1747 indicates the probable procedure for keeping the general public out. Subscriptions are declared to be acceptable from " all persons ", with the proviso, however, of the " Subscribers being Persons with whose Abilities the said Governor and Company are well satisfied ". (*A Preamble to the Books for taking a Subscription for Eighteen Hundred Thousand Pounds, for Support of the Credit of the Bank of England, and for circulating Exchequer Bills*, 1747, No. 37. In Kress Library, Harvard University.)

Sir John relates that there were two 20 per cent. calls on subscribers in 1714, without further explanation (I, 69). The account given in a later tract throws some light on how the Subscription worked. In January, 1714, a rumour was about of an impending French invasion: " Now though this was entirely a Fiction, yet the Dread of its Reality so affected the Nation, that in an Instant the Public Securities fell, and a prodigious Run was made upon the Bank, who call'd in forty *per* Cent. from the Subscribers to the circulating of the *Exchequer* Bills, and deputed four of their Directors to wait upon the Treasurer in this Exigency to represent the Danger they were under . . . (*A Letter from a Bystander to a Member of Parliament*, 1741/2, p. 10).

The account given here (I, 85–90) of the abortive negotiations for assistance by the Bank to the South Sea Company during the crisis in 1720–21 in the latter's affairs mentions Robert Walpole only in connection with the later settlement by arbitration of the claims of the Company against the Bank. A pamphlet written by or on behalf of Walpole which makes use of the original records of the negotiations (*Some Considerations concerning the Publick Funds*, 1735, pp. 85–91) reveals that he had an important rôle in these negotiations from the start, and indicates also that it was only under strong pressure from the Government that the Bank entered into the negotiations at all.

The statement *re* the Act of May 10, 1774, that " the decision of Government to deal with the gold [recoinage] and leave the silver in its existing state—and that could not have been much worse—was in effect the introduction of a gold standard " (I, 176) gives, I think, a misleading impression of the significance of the Act. Before 1774, both gold and silver coin were unlimited legal tender, but silver, at 5/2 per ounce, was undervalued, and gold was already the effective standard. Under the Act of 1774 silver coin was limited as legal tender by tale to £25, but beyond that sum it was still unlimited legal tender by weight at 5/2 per ounce. At that price full-weight silver coin would not have stayed in circulation before or after the Act, and the failure to recoin the silver had the effect, probably intended, of keeping it in circulation rather than of driving it out.

The Phineas Bond " of Philadelphia " cited as an *American* holder of Bank stock in 1801 (I, 289) was British Consul-General at Philadelphia. He had much to say on the damage to British interests allegedly resulting from the flow of British capital to the United States. (Cf. *Annual Report American Historical Association for 1897*, 1898, pp. 536–37.)

We are told that " The relation of the Scottish banking organisation to the English was peculiar, indirect, but important " in the eighteenth century and that " This power [of the Scotch banks] of drawing, directly or indirectly, *on the Bank's gold* for Scottish use was only important in difficult times " (I, 167, 168, italics mine). But Scotland was really on a *de facto* sterling exchange standard in the eighteenth—and nineteenth—century, very much as Canada was on a *de facto* U.S. dollar exchange standard before 1914. To the Scotch banks their power to draw on London to meet adverse balances in England was probably much more important than their power to draw *gold* from England to Scotland, where there would be little call for it even in difficult times.

The reference to Sir John Sinclair as " the reputed inventor of the Exchequer Bill cure for the evils of a crisis ", *i.e.*, the special issue of exchequer bills against merchandise or other collateral to relieve a credit crisis (II, 34), is no doubt correct, but I can find no evidence of any such proposal by Sinclair prior to 1784, and the device seems to have been resorted to at least as early as 1782. This use of exchequer bills has theoretical as well as historical interest and someone should investigate its history.

In connection with the suspension of cash payments by the Bank during the Napoleonic Wars, the Bank is said to have gone on " buying gold as it could, at most unremunerative prices " in 1811—1812 (II, 30). Did not the Bank, however, sell the gold, through its bullion department, at the remunerative market prices? There has been some question about the nature of the gold dealings of the Bank during the suspension, and it would have been interesting to know what light the Bank's records throw on this question. I doubt whether it is correct to speak of Francis Horner as a " friend " of Ricardo as early as 1810 (II, 23). " For their stupidities [*i.e.*, those of Vansittart's Resolutions of 1811] the Bank was not responsible " (II, 28). Perhaps not, but the Bank matched them.

" His [Ricardo's] critics have pointed out that as a stock-jobber, with natural ' bear ' leanings, Ricardo was interested in anything that would bring down the funds, as a curtailment of Bank issues or a return to convertibility most certainly would " (II, 23). The implication here, of course, was that Ricardo's opposition to the Bank was in part at least to be explained by his selfish profit interests. Silberling, it is true, did " point out " this blemish on Ricardo's record, as did also Angell on the basis of Silberling's data. But the only evidence presented by Silberling was derived from an anonymous pamphlet. (*The Bank—The Stock Exchange—The Bankers . . . An Exposé*, 1821.) Clapham elsewhere in this work is very deprecatory of pamphlets as historical sources, especially if they are anonymous and especially if they are unfriendly to the Bank. Even a cursory examination of this particular pamphlet would reveal that it was the product of a disordered mind. In it the routine business of the stock market is pictured as an elaborate succession of wild plots and conspiracies. Ricardo (" the great David ") is described as openly carrying on bear-operations against the Funds. I know of no other evidence to this effect, and if the charge is true I cannot understand why more was not made of it at the time by the defenders of the Bank and the Government, who needed every available argument to offset the effect of Ricardo's criticism, but who nevertheless always referred to Ricardo as if he were a most respectable and upright person. Moreover, it is not clear to me why, since Ricardo was also a loan-contractor and a large fundholder on his own account, it would not have been as " natural " for him to have " bull " leanings. Nor would convertibility " certainly " bring down the funds. The usual effect of an appreciation of a currency is in fact to raise the value of gilt-edge fixed-income securities.

The Bank's records, it is made apparent by Sir John, are very scanty and unrevealing with respect to negotiations and discussions between the Government and the Bank. A thorough study of the relations between the two will not be possible unless there exist manuscript records in the Treasury archives or in the private papers of statesmen and former Bank officials. It may be that for reasons of convenience or better to preserve secrecy much of the discussion was carried on orally. It is more probable, however, that negotiations were usually in writing, but were kept semi-private in character, and that the records thereof were kept as private papers by the participants. In any case, Sir John did not find much in the Bank's records bearing on the negotiations between the Bank and the Government in the nineteenth century. For example, he found nothing in the records of the Bank to support the statement which I had made (without citation of my authority, I regret to say) that early in 1822 the Bank had refused to accede to a request from the Government to lower its discount rate. A statement to this effect was made by Lord Liverpool in the House of Lords on February 26, 1922 (*Hansard*, New Series, VI [1822], 715–16).

Robert Lowe, in June, 1873, introduced a Bill to authorise temporary increases in certain contingencies of the amount of Bank of England notes which could be issued in exchange for securities. We are told that " no opinions from the Bank on Lowe's proposals have survived " (II, 288). It may be prudent to read this as " have survived in the records of the Bank itself ". James Pennington for many years acted as an adviser to the Chancellor of the Exchequer on currency and banking matters. In the University of Chicago Library there are some fragmentary copies of correspondence between Pennington, on the one hand, and Arbuthnot, Trevelyan, and Goulburn, all associated in one connection or another with the Treasury, on the other, dealing with Bank of England matters. This correspondence shows that a proposal with some resemblance to Lowe's had been made by William Cotton, the then Governor of the Bank, to Goulburn in 1845, and was again under consideration in 1855. One item in this correspondence helps incidentally to explain the poverty of the Bank's records. Goulburn, in transmitting Cotton's letter to Pennington, writes: " I make this communication in perfect confidence,—the letter of the Governor to me, being more in the character of a private than an official letter ".

Section 9 of the Bank Act of 1844 provided that if the Bank took over the note issues of other banks, it was to make additional payments to the Treasury " equal in amount to the net profit derived in the said issue department . . . from such additional issues after deducting the amount of the Expenses occasioned by the additional issue ". This correspondence reveals that the interpretation of this provision gave rise to rather sharp discussions between the Treasury and the Bank. The Bank insisted that the deduction for expense should be on the basis of the cost, presumably the average cost, of issue through the Branches, where the cost was higher than at the Head Office. The Bank also asked for a guarantee against any risk of loss at the expiration of the Charter from a fall in the price of the securities held as cover for the additional issues. Goulburn, on the other hand, at least at first, held that the revenues and costs connected with the additional issues should be computed on what was in effect a marginal revenue and a marginal cost basis, and insisted that the chance of loss with respect to the securities be treated as " a banking risk " and as " part of the price paid by the Bank of England for its privileges ". It would be interesting to know the dénouement of this controversy, which raises a nice issue in cost-accounting, and also reflects both the characteristic parsimony of the Treasury in its relations with the Bank and the constant and anxious concern of the Bank with respect to its revenues.

On these and other matters, we can safely take for granted that not much more could have been done on the basis of the Bank's own records than the work under review has done. For further light on these aspects of the pre-1914 history of the Bank, we must await the discovery and exploitation of relevant material in the papers of the Treasury and in the private papers of Bank and Government officials.

The Poverty of Historicism, III

By Karl Popper

(27) *Is there a Law of Evolution? Laws and Trends.* The claims of historicism which I have called *pro-naturalistic* and to which we now turn our attention have much in common with its anti-naturalistic claims; for example, they are influenced by holistic thinking, and they spring from a misunderstanding of the methods of the natural sciences. Since they represent a misapplied effort to copy these methods, they may be described as " scientistic " (in Professor Hayek's sense[1]). They are just as characteristic of historicism as are its anti-naturalistic claims, and perhaps even more important. The view, more especially, that it is the task of the social sciences to lay bare the *law of evolution* of society in order to foretell its future (a view developed in sections 14 to 17, above) might be described as the most central of all historicist doctrines. For it is this view of a society moving through a series of periods that gives rise, on the one hand, to the contrast between a changing social and an unchanging physical world and thereby to anti-naturalism, and on the other hand to the pro-naturalistic and scientistic belief in so-called " natural laws of succession "; a belief which, in the times of Comte and Mill, could claim to be supported by the long-term predictions of astronomy, and more recently, by Darwinism. The recent vogue of historicism may indeed be said to have been merely part of the vogue of evolutionism—of a philosophy that owed its influence largely to the somewhat sensational clash[2] between a brilliant scientific hypothesis concerning the history of the various species of animals and plants on earth, and an older metaphysical theory which, accidentally, happened to be an established religious belief.

What we call the evolutionary hypothesis[3] is an explanation of a host of biological and paleontological observations—for instance, of certain similarities between various species and genera—by the assumption of the common ancestry of related forms. This hypothesis is not a universal law, even though certain universal laws of nature, such as laws of heredity, segregation, and mutation, enter with it into the explanation. It has, rather, the character of a singular or specific historical statement. (It is of the same status as the historical statement: " Charles Darwin and Francis Galton had a common grandfather ".) The fact that the evolutionary hypothesis is not a universal law of nature[4] but a singular historical statement about the ancestry of a number of terrestrial plants and animals is somewhat obscured by the fact that the term " hypothesis " is so often used to characterise the status of universal laws of nature. But we should not forget that we quite frequently use this term in a wider sense. For example, it would undoubtedly be correct to describe a tentative medical diagnosis as a hypothesis, even though such a hypothesis is of a singular and historical character rather than of the character of a universal law. In other words, the fact that all laws of nature are hypotheses must not distract our attention from the fact that not all hypotheses are laws, and that historical hypotheses more especially are, as a rule, not universal but singular statements about one individual event, or a number of such events.

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[1] Cp. F. A. v. Hayek, " Scientism and the Study of Society," Economica, N.S., Vol. IX, esp. p. 269. Professor Hayek uses the term scientism as a name for " the slavish imitation of the method and language of science "; here it is used, rather, as a name for the imitation of *what certain people mistake* for the method and language of science.

[2] I agree with Professor Raven when, in his *Science, Religion, and the Future* (1943), he calls this conflict " a storm in a Victorian tea-cup "; though the force of this remark is perhaps a little impaired by the attention he pays to the vapours still emerging from the cup—to the Great Systems of Evolutionist Philosophy produced by Bergson, Whitehead, Smuts, and others.

[3] Because of the tendency of evolutionists to suspect of obscurantism anyone who does not share their emotional attitude towards evolution as a " daring and revolutionary challenge to traditional thought ", I may perhaps mention that I see in modern Darwinism the most successful explanation of the relevant facts. A good illustration of the emotional attitude of evolutionists is Dr. Waddington's statement (*Science and Ethics*, 1942, p. 17) that " we must accept the direction of evolution as good simply because it *is* good . . ."; a statement which also illustrates the pertinence of Professor Bernal's remark (*ibid.*, p. 115): " It was not . . . that science had to fight an external enemy, the Church; it was that the Church . . . was within the scientists themselves ".

[4] Even a statement such as " All vertebrata have one common pair of ancestors " is not, in spite of the word " all ", a universal law of nature; for it refers to the vertebrata existing on earth, rather than to all organisms at any place and time which have that constitution which we consider as characteristic of vertebrata. Cp. my *Logik der Forschung*, sections 14f.

But can there be a *law* of evolution? Can there be a scientific law in the sense understood by T. H. Huxley when he wrote:[5] "... he must be a half-hearted philosopher who ... doubts that science will sooner or later ... become possessed of the law of evolution of organic forms—of the unvarying order of that great chain of causes and effects of which all organic forms, ancient and modern, are the links ..."?

I believe that the answer to this question must be "no", and that the search for the law of the "unvarying order" in evolution cannot possibly fall within the scope of scientific method, whether in biology or in sociology. The reasons for this belief are very simple. The evolution of life on earth, or of human society, is a unique historical process. Such processes, we may assume, proceed in accordance with all kinds of causal laws, e.g., the laws of mechanics, of chemistry, of heredity and segregation, of natural selection, etc. They cannot, however, be desribed by a law, but only by a singular historical hypothesis. Universal laws make assertions concerning some unvarying order, as Huxley puts it, i.e., concerning all processes of a certain kind; and although there is no reason why the observation of one single instance should not incite us to formulate a universal law, and even why, if we are lucky, we should not hit the truth, it is clear that any law, formulated in this or in any other way, must be *tested* by new instances before it can be taken seriously by science. In other words, we cannot hope to test a universal hypothesis nor to find a natural law acceptable to science if we are confined to the observation of one unique process. Nor can the observation of one unique process help us to foresee future developments. The most careful observation of *one* developing caterpillar will not help us to predict its transformation into a butterfly. As applied to the history of human society—and it is with this that we are mainly concerned here—our argument has been formulated by H. A. L. Fisher[6] in these words: "Men ... have discerned in history a plot, a rhythm, a predetermined pattern ... I can see only one emergency following upon another ..., *only one great fact with respect to which, since it is unique, there can be no generalisations ...*"

How can this simple argument be countered? There are, in the main, two positions which may be taken up by those who believe in a law of evolution. They may (*a*) deny our contention that the evolutionary process is unique; or (*b*) assert that in an evolutionary process, even if it is unique, we may discern a trend or tendency or direction, and that we may formulate a hypothesis which states this trend, and test this hypothesis by future experience. The two positions (*a*) and (*b*) are not exclusive of each other.

Position (*a*) goes back to an idea of great antiquity—the idea that the life-cycle of birth, childhood, youth, maturity, old age, and death, applies not only to individual animals and plants, but also to societies, races, and perhaps even to "the whole world". This ancient doctrine was used by Plato in his interpretation of the decline and fall of the Greek city states and of the Persian Empire.[7] Similar applications have been made by Machiavelli, Vico, O. Spengler, and recently by Professor Toynbee in his fascinating *Study of History*. From the point of view of this doctrine, history is repetitive, and the laws of the life-cycle of civilisations, for instance, can be studied in the same way as we study the life-cycle of a certain animal species. It is a consequence of this doctrine, although one which its originators hardly intended,[8] that our objection, based on the uniqueness of the evolutionary or historical process, loses its force. Now I do not intend to deny (nor, I feel certain, did Professor Fisher in the passage quoted) that history may sometimes repeat itself in certain respects, nor that the parallelism between certain types of historical events, such as the rise of tyrannies in ancient Greece and in modern times, can be significant for the student of the sociology[9] of political power. But it is clear that all these instances of repetition involve circumstances which are vastly dissimilar, and which may exert an important influence upon further developments. We therefore have no valid reason to expect of any apparent repetition of a historical development that it will *continue* to run parallel to its prototype. Admittedly, once we believe in a law of repetitive life-cycles—a belief arrived at by analogical speculations, or perhaps inherited from Plato—we are sure to discover historical confirmation of it nearly everywhere. But this is merely one of the many instances of metaphysical theories seemingly confirmed by facts which, if examined more closely, turn out to be selected in the light of the very theory they are supposed to test.[10]

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[5] Cp. T. H. Huxley, *Lay Sermons* (1880), p. 214. Huxley's belief in a law of evolution is very remarkable in view of his exceedingly critical attitude towards the idea of a law of (inevitable) progress. The explanation appears to be that he not only distinguished sharply between natural evolution and progress, but that he held (rightly, I believe) that these two had little to do with each other. Julian Huxley's interesting analysis of what he calls "evolutionary progress" (*Evolution*, 1942, 559ff) seems to me to alter little in this, although it is apparently designed to establish a link between evolution and progress. For he admits that evolution, though sometimes "progressive" is more often not so. (For this, and for Huxley's definition of "progress", see note 5 to section 28.) The fact, on the other hand, that every "progressive" development may be considered as evolutionary, is hardly more than trivial. (That the succession of dominant types is progressive in his sense may merely mean that we habitually apply the term "dominant types" to those of the most successful types which are the most "progressive".)

[6] Cp. H. A. L. Fisher's *History of Europe*, Vol. I, p. VII (italics mine). See also F. A. v. Hayek, *op. cit.*, ECONOMICA, Vol. X, p. 58, who criticises the attempt "to find laws where in the nature of the case they cannot be found, in the succession of the unique and singular historical phenomena".

[7] Plato describes the cycle of the Great Year in *The Statesman*; proceeding from the assumption that we live in the season of degeneration, he applies this doctrine in *The Republic* to the evolution of Greek cities, and in *Laws* to the Persian Empire.

Turning to position (*b*), i.e., the belief that we may discern, and extrapolate, the trend or direction of an evolutionary moment, it may first be mentioned that this belief has influenced some of the cyclical hypotheses which represent position (*a*). Professor Toynbee, for example, expresses in support of position (*a*) the following views characteristic of (*b*): " Civilisations are not static conditions of society but dynamic movements of an evolutionary kind. They not only cannot stand still, but they cannot reverse their direction without breaking down their own law of motion . . .".[11] Here we have nearly all the elements usually found in statements of the position (*b*) : the idea of social *dynamics* (as opposed to social *statics*); of evolutionary *movements* of societies (under the influence of social *forces*); and of *directions* (and *courses*, and *velocities*) of such movements which, it is said, cannot be reversed without breaking the *laws of motion*. The terms in italics have all been taken over from physics into sociology, and their adoption has led to a series of misunderstandings which are of a rather astonishing crudity, but very characteristic of the scientistic misuse of the examples of physics and astronomy. Admittedly, these misunderstandings have done little harm outside the range of historicist endeavours. In economics, for example, the use of the term " *dynamics* " (cp. the now fashionable term macro-dynamics) is unobjectionable, as must be admitted even by those who dislike the term. But even this use derives from Comte's attempt to apply to sociology the physicist's distinction between statics and dynamics; and there can be no doubt of the gross misunderstanding that underlies this attempt. *For the kind of society which the sociologist calls " static " is precisely analogous to such physical systems as the physicist would call " dynamic "*. A typical example is the solar system; it is the prototype of a dynamic system in the sense of the physicist; but since it is repetitive, since it does not grow or develop, since it does not show any structural changes (apart from such changes as do not fall within the realm of celestial dynamics and which may therefore be neglected here), it corresponds, undoubtedly, to those social systems which the sociologist would call " static ". The point is of considerable importance in connection with the claims of historicism, in so far as the success of the long-term predictions of astronomy depends entirely on this repetitive, and in the sociological sense static, character of the solar system—on the fact that we may here neglect any symptoms of a historical development. It is therefore certainly a mistake to consider these predictions as establishing the possibility of large-scale historical prophecies.

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[8] Professor Toynbee insists that his method is to investigate empirically the life cycle of 21 odd specimens of the biological species " civilisation ". But even he appears not to be influenced, in his adoption of this method, by any desire to counter Fisher's argument (quoted above); at least, I do not find any indication of such a desire in his comments on this argument which he is content to dismiss as an expression of " the modern Western belief in the omnipotence of chance " ; cp. *A Study of History*, Vol. V, p. 414. I do not think that this characterisation does justice to Fisher who says in the continuation of the passage quoted : ". . . The fact of progress is written plain and large on the page of history ; but progress is not a law of nature. The ground gained by one generation may be lost by the next."

[9] In biology, the position is similar in so far as a multiplicity of evolutions (e.g., of different genera) may be taken as the *basis of generalisations*. But this comparison of evolutions has merely led to the description of *types* of evolutionary processes. The position is the same as in social history. We may find that certain types of events are repeated here or there, but no law describing either the course of all evolutionary processes (such as a law of evolutionary cycles) or the course of evolution in general appears to result from such a comparison. Cp. note 5 to section 28.

[10] Of nearly every theory it may be said that it agrees with some facts : this is one of the reasons why a theory should be said to be confirmed if we are unable to discover refuting facts, rather than if we are able to find supporting facts ; cp. section 29, below, and my *Logik der Forschung*, esp. Ch. VIII.—An example of the procedure criticised here is, I believe, Professor Toynbee's apparently empirical investigation into the life cycle of what he calls the " species civilisation " (cp. note 8 to this section). He seems to overlook the fact that he classifies as civilisations only such entities as conform to his *a priori* belief in life cycles. For example, Professor Toynbee contrasts (*op. cit.*, Vol I, pp. 147 to 149) his " civilisations " with " primitive societies " in order to establish his doctrine that these two cannot belong to the same " species " although they may belong to the same " genus ". But the only basis of this classification is an *a priori* intuition into the nature of civilisations. This may be seen from his argument that the two are obviously as different as are elephants from rabbits—an intuitive argument whose weakness becomes clear if we consider the case of a St. Bernard dog and a Pekinese. But the whole question (whether or not the two belong to the same species) is inadmissible, for it is based on the scientistic method of treating collectives as if they were physical or biological bodies. Although this method has often been criticised (cp. for example F. A. v. Hayek, Economica, Vol. X, p. 41ff) these criticisms have never received an adequate reply.

[11] Toynbee, *op. cit.*, Vol. I, p. 176.

Very similar misunderstandings are involved in the application to society of the other terms of physics listed above. Often this application is quite harmless. No harm is done, for example, if we describe changes in social organisation, in the methods of production, etc., as of *movements*. But we ought to be clear that we are simply using a metaphor, and a rather misleading one. For if we speak in physics of the movement of a body or of a system of bodies then we do not intend to imply that the body or system in question undergoes any internal or structural change, but only that it changes its position relative to some (arbitrarily chosen) system of co-ordinates. As opposed to this, the sociologist means by a " movement of society " some structural or internal change. He will, accordingly, assume that a movement of society is to be explained by *forces*[12] while the physicist assumes that only *changes* of movement, but not movement as such, have to be so explained. The ideas of the *speed* of a social movement, or of its *track*, or *course*, or *direction*, are similarly harmless as long as they are used merely in order to convey some intuitive impression ; but if used with anything like scientific pretention, they simply become scientistic jargon, or more precisely, holistic jargon. Admittedly, any kind of change of a measurable social quality, for example, population growth, may be graphically represented as a track, just like the path of a moving body; but it is clear that such a diagram does not depict what people mean by the movement of society; consider that, in our example, a stationary population may undergo a radical social upheaval. We may, of course, combine any number of such diagrams into one single multi-dimensional representation. But such a combined diagram cannot be said to represent the path of the movement of society; it does not tell us more than do the single ones together; it does not represent any movement of " the whole society ", but only changes of selected aspects. The idea of the movement of society itself—the idea that society, like a physical body, can move *as a whole* along a certain path, and in a certain direction—is merely a holistic confusion.[13]

The hope, more especially, that we may some day find the " laws of motion of society ", just as Newton found the laws of motion of physical bodies, is nothing but the result of these misunderstandings. Since there is no motion of society in any sense similar or analogous to the motion of physical bodies, there can be no such laws.

But, it will be said, the existence of trends or tendencies in social change can hardly be questioned: every statistician can calculate such trends. Are these trends not something comparable with Newton's law of inertia ? The answer is: trends exist, or more precisely, the assumption of trends is often a useful statistical device. *But trends are not laws.* A statement asserting the existence of a trend is existential,

[12] For an example of a typically " scientistic " attempt to compute political " forces " with the help of the Pythagorean theorem, cp. note 6 to section 20, above (ECONOMICA, Vol. XI, p. 122).

[13] The confusion created by the talk about " motion ", " force ", " direction ", etc., may be gauged by considering that Henry Adams, the famous American historian, seriously hoped to determine the course of history by fixing the position of two points on its track—the one point located in the thirteenth century, the other in his own life-time. He says himself of his project : " With the help of these two points . . . he hoped to project his lines forward and backward indefinitely . . ." ; for, he argued, " any schoolboy could see that man as a force must be measured by motion, from a fixed point ". (*The Education of Henry Adams*, 1918, p. 434f.). As a more recent example, I may quote Waddington's remark (*Science and Ethics*, p. 17f.) that " a social system " is " something the existence of which essentially involves motion along an evolutionary path . . .", and that (p. 18f.) " the nature of science's contribution to ethics " . . . is the revelation of the nature, the character and direction of the evolutionary process in the world as a whole . . ."

not universal. (A universal law, on the other hand, does not assert existence; on the contrary: as was shown at the end of section 20, it asserts the non-existence of something or other.[14]) And a statement asserting the existence of a trend at a certain time and place would be a singular historical statement, not a universal law. The practical significance of this logical situation is considerable: while we may base scientific predictions on laws, we cannot (as every cautious statistician knows) base them merely on the existence of trends. A trend (we may again take population growth as an example) which has persisted for hundreds or even thousands of years may change within a decade, or even more rapidly than that.

It is important to point out that *laws and trends are radically different things.*[15] For undoubtedly, the habit of confusing trends with laws, together with the intuitive observation of trends (such as technical progress), inspired the central doctrines of evolutionism and historicism—the doctrines of the inexorable laws of biological evolution and of the irreversible laws of motion of society. And the same confusions and intuitions also inspired Comte's doctrine of laws of succession—a doctrine which is still very influential.

The distinction, famous since Comte and Mill, between *laws of coexistence*, corresponding to statics, and *laws of succession*, corresponding to dynamics, can admittedly be interpreted in a reasonable way; i.e., as a distinction between laws in whose formulation *time* enters (for instance laws that speak of velocities), and laws that do not involve the concept of time.[16] But this is not quite what Comte and his followers had in mind. When speaking of laws of succession, Comte thought of laws determining the succession of a " dynamic " series of phenomena in the order in which we observe them. Now it is important to realise that " dynamic " laws of succession, as Comte thought of them, simply do not exist. The closest approach to them, and what he probably had in mind, are natural periodicities like the seasons, the phases of the moon, or eclipses, or perhaps the swings of a pendulum. But these periodicities are static, not dynamic, in Comte's sense of these terms, and in any case can hardly be called laws (since they depend upon the special conditions prevailing in the solar system; cp. next section). We shall call them " quasi-laws of succession ".

The crucial point is this: although we may assume that any actual succession of phenomena proceeds according to the laws of nature, it is important to realise that practically no series of, say, three or four causally connected phenomena proceeds according to any one law of nature. If the wind shakes a tree and Newton's apple falls to the ground, nobody would deny that these events can be described in terms of causal laws. But there is no *one* law, such as that of gravity, nor one definite set of laws, to describe the succession of events; apart from gravity we would have to consider the laws explaining wind pressure; the jerking movements of the branch; the tension in the apple's stalk; the bruise suffered by the apple on impact; the succeeding chemical processes; etc. The idea that any concrete series or succession of phenomena (apart from such examples as the movement of a pendulum or a solar system) can be described or explained by any one law, or by any definite set of laws, is simply mistaken. There are neither laws of succession, nor laws of evolution.

That Comte and Mill indeed envisaged their historical laws of succession as laws determining a series of historical events in the order of their actual occurrence may be seen from the manner in which Mill speaks of a method that " consists in attempting, by a study and analysis of the general facts of history, to discover . . . the law of progress; which law, once ascertained, must . . . enable us to predict future events, *just as after a few terms of an infinite series in algebra we are able to detect the principle of regularity in their formation, and to predict the rest of the series to any number of terms we please* ".[17] Mill himself is critical of this method; but his criticism (see the beginning of section 28) fully admits the possibility of finding laws of succession analogous to those of a mathematical series,[18] even though he expresses doubts whether " the order of succession . . . which history presents to us " is " rigidly uniform " enough to be compared with a mathematical series.

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[14] See my *Logik der Forschung*, p. 32f., where reasons are given for considering existential statements to be *metaphysical* (in the sense of unscientific); see also note 7 to section 28, below.

[15] A law, however, may assert that under certain circumstances (initial conditions) certain trends will be found; Moreover, after a trend has been so explained, it is possible to formulate a law corresponding to the trend; see also note 8 to section 28, below.

[16] It may be worth mentioning that equilibrium economics is undoubtedly *dynamic* (in the " reasonable " as opposed to the " Comtean " sense of this term), even though time does not occur in its equation. For this theory does not assert that the equilibrium is anywhere realised; it merely asserts that every disturbance (and disturbances occur all the time) is followed by an adjustment—by a " movement " towards equilibrium.

[17] Mill, *Logic*, Book VI, Ch. X, section 3. For Mill's theory of " progressive effects " in general, see also Book III, Ch. XV, sections 2f.

Now we have seen that there are no *laws* that determine the succession of such a " dynamic " series of events.[19] On the other hand, there may be *trends* which are of this " dynamic " character; for example, population increase. It may, therefore, be suspected that Mill had such trends in mind when he spoke of " laws of succession ". And this suspicion is confirmed by Mill himself when he describes his historical law of progress as a *tendency*. Discussing this " law ", he expresses his " belief . . . that the general *tendency* is, and will continue to be, saving occasional and temporary exceptions, one of improvement—*a tendency towards a happier and better state*. This . . . is . . . a theorem of the science " (viz., of the social science).—It is in keeping with this fundamental confusion between laws and trends, as well as with the holistic confusion, that Mill should seriously discuss the question whether " the phenomena of human society " revolve " in an orbit " or whether they move, progressively, in " a trajectory ".[20]

In order to avoid misunderstandings, I wish to make it clear that I believe that both Comte and Mill have made great contributions to the philosophy and methods of science; I am thinking, especially, of Comte's emphasis on laws and scientific prediction, of his criticism of an essentialist theory of causality; and of his and Mill's doctrine of the unity of scientific method. Yet their doctrine of historical laws of succession is, I believe, little better than a collection of misapplied metaphors.[21]

(28) *The Method of Reduction. Causal Explanation. Prediction and Prophecy.* Our criticism of the doctrine of historical laws of succession is in one important respect still inconclusive. We have tried to show that the " directions " or " tendencies " which historicists discern in the succession of events called history are not laws but, if anything, trends. And we have pointed out why a trend, as opposed to a law, may not in general be used as a basis for scientific predictions. But to this criticism, Mill and Comte—alone in this respect among historicists, I believe—could still offer a rejoinder. Mill might perhaps have admitted a certain amount of confusion between laws and trends, but he could remind us that he himself had criticised those who mistook a " uniformity of historical succession " for a true law of nature; that he had been careful to emphasise that such a uniformity could " only be an empirical law "[1] (the term is somewhat misleading) and that it should not be considered secure before it was reduced, " by the consilience of deduction *a priori* with historical evidence ", to true laws of nature; and that he had even announced[2] the " imperative rule never

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[18] Mill seems to overlook the fact that only the very simplest arithmetical and geometrical series are such that " a few terms " suffice for detecting their " principle ". It is possible to construct, more complicated mathematical series in which thousands of terms would not suffice to discover their law of construction—*even if it is known that there is such a law.*

[19] For the nearest approach to such laws, see section 28, esp. note 6.

[20] Cp. Mill, *loc. cit.*, Mill distinguishes two senses of the word " progress " ; in the wider sense, it is opposed to cyclic change but does not imply improvement. (He discusses " progressive change " in this sense more fully in *op. cit.*, Book III, Ch. XV.) In the narrower sense, it implies improvement. He teaches that the persistence of progress in the wider sense is a question of *method* (I do not understand this point), and in the narrower sense a theorem of sociology.

[21] In many historicist and evolutionist writings it is often impossible to discover where the metaphor ends and the seriously meant theory begins. (See for example, notes 10, 12 and 13 to the present section.) And we must even face the possibility that some historicists may deny that there is a difference between metaphor and theory; for such a possibility is indicated in the following quotation from the psycho-analyst Dr. Karin Stephen : " That the modern explanation which I have tried to put forward may still be no more than a metaphor I will concede . . . I do not think we need be ashamed . . . because scientific hypotheses are in fact all based on metaphor. What else is the wave theory of light . . . ? (Cp. Waddington's *Science and Ethics*, p. 80 ; see also p. 76 on gravity.) If the method of science were still that of essentialism, the method of asking " what *is* it " ? (Cp. section 10 above), and if the wave theory of light were the essentialist statement that light *is* a wave motion, then this remark would be justified. But as things are, it is one of the main differences between psycho-analysis and the wave theory of light that while the former is still largely essentialistic and metaphysical, the latter is not.

[1] This and the next quotation are from Mill, *Logic*, Book VI, Ch. X, section 3. I consider the term " empirical law " (used by Mill as a name for a law of a low degree of generality) as very unfortunate because *all* scientific laws are empirical : they all are accepted or rejected on the basis of empirical evidence. (For Mill's " empirical laws ", see also *op. cit.*, Book III, Ch. VI, and Book VI, Ch. V, section 1. Mill's distinction has been accepted by C. Menger who opposes " exact laws " to " empirical laws " ; cp. *The Collected Works*, Vol. II, pp. 38ff., and 259ff.

[2] Cp. Mill, *op. cit.*, Book VI, Ch. X, section 4. See also Comte, *Cours de philosophie positive*, IV, p. 335.

to introduce any generalisation from history into the social science unless sufficient grounds can be pointed out for it ", viz., by deducing it from some true natural laws (the laws he had in mind were those of " human nature ", i.e., psychology) which can be established independently. To this procedure of reducing historical or other generalisations to some set of laws of higher generality, Mill gave the name " inverse deductive method ", and he advocated it as the only correct historical and sociological method.

Now it must be admitted that there is a great deal in this rejoinder. For should we succeed in reducing a trend to a set of laws, then we should be justified in using this trend, like a law, as a basis of predictions. Such a reduction, or inverse deduction, would go a long way towards bridging the gulf between laws and trends. On the other hand, Mill's method of inverse deduction is a fair (although scrappy) description of a procedure which is not only used in the social sciences but in all sciences, and to an extent far beyond Mill's own estimate.

In spite of these admissions I believe that our criticism remains correct, and that the fundamental historicist confusion of laws with trends is indefensible. But in order to show this, a careful analysis is needed of the method of reduction or inverse deduction.

Science, we may say, is confronted with problems at any moment of its development. It cannot start with observations, or with the " collection of data ", as some students of method believe. Before we can collect data, our interest in *data of a certain kind* must be aroused: the *problem* always comes first. The problem in its turn may be suggested by practical needs, or by scientific or pre-scientific beliefs which, for some reason or other, appear to be in need of revision.

Now a scientific problem, as a rule, is the need for an *explanation.* Following Mill, we shall distinguish between two main cases: the explanation of an individual or singular specific event, and that of some regularity or law. Mill puts it as follows: " An individual fact is said to be explained by pointing out its cause, that is, by stating the law or laws . . . of which its production is an instance. Thus a conflagration is explained when it is proved to have arisen from a spark falling into a heap of combustibles; and in a similar manner, a law . . . is said to be explained when another law or laws are pointed out, of which that law itself is but a case, and from which it could be deduced ".[3] The case of the explanation of a law is a case of inverse deduction, and therefore important in our context.

Mill's explanation of an explanation, or better, of a causal explanation, is in the main quite acceptable. But for certain purposes it is not precise enough; and this lack of precision plays an important part in the issue we are here concerned with. I shall therefore re-state the matter, and point out where the differences lie.

I suggest that to give a causal explanation of a certain *specific event* means deducing a statement describing this event from two kinds of premises, viz., from some *universal laws*, and from some singular or specific statements which we may call the *specific initial conditions.* For example, we can say that we have given a causal explanation of the breaking of a certain thread if we find that this thread could carry a weight of only one pound, and that a weight of two pounds was put on it. If we analyse this causal explanation, then we find that two different constituents are involved. (1) We assume some hypotheses of the character of universal laws of nature; in this case, perhaps: " Whenever a certain thread undergoes a tension exceeding a certain minimum tension, characteristic for that particular thread, then it will break ". (2) We assume some specific statements (the initial conditions) pertaining to the particular event in question; in this case, we may have two statements: " For this thread, the characteristic minimum tension at which it is liable to break is equal to a one pound weight " and " the weight put on this thread was a two pound weight ". Thus we have two different kinds of statements which together yield a complete causal explanation, viz.: (1) *universal statements of the character of natural laws;* and (2) *specific statements pertaining to the special case in question, the initial conditions.* Now from the universal laws (1), we can deduce with the help of the initial conditions (2) the following specific statement (3): " This thread will break ". This conclusion (3) we may also call a specific *prognosis.*—The initial conditions (or more precisely, the situation described by them) are usually spoken of as the *cause* of the event in question, and the prognosis (or rather, the event described by the prognosis) as the effect: for example, we say that the putting of a weight of two pounds on a thread capable of carrying only one pound was the cause of the breaking.[4]

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[3] Mill, *op. cit.*, Book III, Ch. XII, section 1. For the " derivation " or " inverse deduction " of what he calls " empirical laws " see also *lib. cit.*, Ch. XVI, section 2.

Such a causal explanation will be, of course, scientifically acceptable only if the universal laws are well tested and confirmed, and if we have also some independent evidence of the cause described by the initial conditions.

Before proceeding to analyse the causal explanation of regularities or laws, it may be remarked that several things emerge from our analysis of the explanation of singular events. One is that we can never speak of cause and effect in an absolute way, but that an event is a cause of another event—its effect—relative to some universal law. However, these universal laws are very often so trivial (as in our example) that as a rule we take them for granted instead of making conscious use of them. A second point is that the use of a theory for *predicting* some specific event is just another aspect of its use for *explaining* such an event. And since we test a theory by comparing the events predicted with those actually observed, our analysis also shows how theories can be *tested.* Whether we use a theory for the purpose of explanation, of prediction, or of testing, depends on our interest, and on which statements we consider as given or unproblematic and which need testing, etc. (See section 29.)

The causal explanation of a *regularity* described by a universal law is somewhat different from that of a singular event. At first sight, one might think that the case is analogous and that the law in question has to be deduced from (1) some more general laws, and (2) certain conditions, corresponding to the initial conditions which though not singular refer to a certain kind of situation. This, however, is not the case, for the conditions (2) must be explicitly stated in the formulation of the law itself, i.e., the law to be explained; for otherwise the law would be palpably false. (For example, if, with the help of Newton's theory we wish to explain the law that all planets move in elipses, then we have to put explicitly in the formulation of this law the conditions under which we can assert its validity, perhaps in the form : *If* a body moves round a central body of much greater mass on a closed track solely under the influence of the force of gravity, *then* it moves in an elipse with the central body approximately in the one focus.) In other words, the formulation of a universal law has to incorporate all the conditions of its validity, since otherwise we cannot assert it universally, or as Mill says, unconditionally. Accordingly, the causal explanation of a regularity consists in the deduction of a law (containing the conditions under which this regularity holds) from a set of more general laws which have been tested and confirmed independently.

If we now compare our explanation of causal explanation with Mill's we see that there is not much difference as far as the reduction of laws to more general laws is concerned, that is to say, in the causal explanation of regularities. But in Mill's discussion of the causal explanation of *singular events*, there is no clear distinction between (1) the universal laws, and (2) the specific initial conditions. This is, largely, due to Mill's lack of clarity in his use of the term " cause " by which he means sometimes singular events, and sometimes universal laws. We shall now show how this affects the explanation or reduction of trends.

That it is logically possible to reduce or explain trends cannot be doubted. Let us assume, for example, that we find that all planets progressively approach the sun. The solar system would then be a dynamic system in Comte's sense; it would have a development or a history, with a definite trend. The trend could be easily explained in Newtonian physics by the assumption (for which we might find independent evidence) that inter-planetary space is filled with some resisting matter—for example, a certain gas. This assumption would be a new specific initial condition which we would have to add to the usual initial conditions stating the positions and momenta of the planets at a certain time. As long as the new initial condition persists, we shall have a systematic change or trend. Now if we further assume the change to be large, then it must have a very marked systematic influence on the biology and history of the various species on earth, including human history. This shows how we could, in principle, explain certain evolutionary and historical trends—even "general, trends", i.e., trends that persist throughout the development under consideration. It is obvious that these trends would be analogous to the quasi-laws of succession (seasonal periodicities, etc.) mentioned in the previous section, with the difference that they would be "dynamic". They would, therefore, correspond even more closely than these "static" quasi-laws to Comte's and Mill's vague idea of evolutionary or historical laws of succession. Now if we have reason to assume the persistence of the relevant initial conditions then we can, clearly, assume that these trends or "dynamic quasi-laws" will persist, so that they may be used, like laws, as a basis for predictions.

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[4] The paragraph containing the analysis of a causal explanation of a specific event is a practically literal translation from section 12 of my *Logik der Forschung* (1935). At present, I feel inclined to suggest a definition of " *cause* " on the basis of Tarski's semantics (which I did not know when this book was written), along the following lines : The (singular) event *A* is called the *true cause* of the (singular) event *B* if and only if from a set of *true* universal statements (laws of nature) a material implication follows whose implicans designates *A* and whose implicate designates *B*.—Similarly, we could define the concept of a " scientifically accepted cause ". For the semantic concept of designation, see Carnap, *Introduction to Semantics* (1942). It appears that the above definition could be improved by using what Carnap calls " absolute concepts ".—For some historical remarks concerning the problem of cause, see note 7 to Ch. XXV of my forthcoming book *The Open Society and Its Enemies.*

There is little doubt that such *explained trends* (as we may call them), or trends which are on the verge of being explained, play a considerable rôle in modern evolutionary theory. Apart from a number of such trends pertaining to the evolution of certain biological forms such as certain shells and rhinos, it appears that a *general* trend towards an increasing number and an increasing variety of biological forms spreading into an increasing range of environmental conditions[5] is becoming explicable in terms of biological laws (together with initial conditions which make certain assumptions regarding the terrestrial environment of organisms and which, together with the laws, imply, for example, the working of the important mechanism called "natural selection").

All this may appear to speak against us, and indeed to support Mill and historicism. But this is not the case. Explained trends do exist, but their persistence depends on the persistence of certain specific initial conditions (which in turn may sometimes be trends).—Now Mill and his brother historicists *overlook the dependence of trends on initial conditions*. They operate with trends as if these were unconditional like laws. Their confusion of laws and trends[6] makes them believe in trends which are unconditional (and therefore general); or, as we may say, in *absolute trends*[7]; for example, in a general historical tendency towards progress—"a tendency towards a better and happier state". And so far as they consider the reduction of their tendencies, they believe that these tendencies can be immediately derived from universal laws alone, such as the laws of psychology (or perhaps of dialectical materialism, etc.).

This, we may say, is the central mistake of historicism. *Its "laws of development" turn out to be absolute trends*, trends which like laws do not depend on initial conditions, and which carry us irresistibly along a certain direction into the future. They are the basis of unconditional *prophecies*, as opposed to conditional scientific *predictions*.

But what about those who see that trends depend on conditions, and who try to find these conditions and to formulate them explicitly? My answer is that I have no quarrel with them; on the contrary. That trends occur cannot be doubted. Therefore we have the difficult task of explaining them as well as we can,[8] i.e., of determining as precisely as possible the conditions under which they persist. (Cp. section 32.)

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[5] For a discussion of evolutionary trends, see J. Huxley, *Evolution* (1942), Ch. IX. Concerning Huxley's theory of Evolutionary Progress (*op. cit.*, Ch. X) it appears to me that all that can be reasonably asserted is this. The general trend towards an increasing variety of forms, etc., leaves room for the statement that "progress" (Huxley's definition is discussed below) occurs sometimes, and sometimes not; that the evolutions of some forms are sometimes progressive, while most of them are not progressive; and that there is no general reason why we should expect that forms will occur in the future which have made further progress. (Cp. Huxley's contention—*op. cit.* Cp., e.g., p. 571—that, if man were wiped out, further progress is in the highest degree improbable. Although his arguments do not convince me, they lead to an implication with which I am inclined to agree; viz., that biological progress is, as it were, something accidental.) Concerning Huxley's *definition* of evolutionary progress as increasing *all-round biological efficiency*, i.e., as increasing control over and independence of the environment, I feel that he has indeed succeeded in adequately expressing the intentions of many who have used this term. Furthermore, the defining terms are not, I admit, anthropocentric; they contain no valuation. And yet, to call an increase in efficiency or in control "progress" appears to me as expressing a valuation; it expresses the belief that efficiency or control is good, and that the spread of life and its further conquest of dead matter is desirable. But it is certainly *possible* to adopt very different values. I therefore do not think that Huxley's claim that he has given an "objective definition" of evolutionary progress, free from anthropomorphism and value judgments, is tenable. (Cp. *op. cit.*, p. 559; also p. 565, which argues against J. B. S. Haldane's view that the idea of progress is anthropocentric.)

[6] That in Mill's case this confusion is largely to be blamed can be seen by an analysis of his *Logic*; see Book III, Ch. XVI.

[7] There are some logical reasons for describing the belief in an absolute trend as unscientific or metaphysical (cp. note 14 to section 27, above). Such a trend may be formulated by a non-specific or generalised existential statement ("There exists such and such a trend"), which we cannot test since no observation of deviations from the trend can disprove this statement; for we can always hope that "in the long run", deviations in the opposite direction will set matters right again.

The point is that these conditions are so easily overlooked. There is, for example, a trend towards an "accumulation of means of production" (as Marx puts it). But we should hardly expect it to persist if the population decreases rapidly; and such a decrease may in turn depend on extra-economic conditions, for example, on a chance invention, or conceivably on the direct physiological (perhaps biochemical) impact of an industrial environment. There are, indeed, countless possible conditions; and in order to be able to examine these possibilities in our search for the true conditions of a trend, we have continuously to imagine conditions under which the trend in question disappears. But this is just what the historicist cannot do. He firmly believes in his favourite trend, and conditions under which it would disappear seem to him utterly unreal. The poverty of historicism, we might say, is one of imagination. The historicist continuously attacks those who cannot imagine a change in their little worlds, but the historicist is himself deficient in imagination since he cannot imagine a change in the conditions of change.

(29) *The Unity of Method.* It has been briefly indicated in the foregoing section that the methods there analysed are widely used and important—more so than Mill, for example, ever thought. These indications will be now further developed, in order to throw some light on the dispute between anti-naturalism and naturalism. The present section discusses the doctrine of the unity of method, that is to say, the doctrine that all theoretical or generalising sciences (we postpone the discussion of the historical sciences) make use of the same method, whether they are now natural sciences or social sciences. At the same time, a number of the claims of historicism which so far have not been examined sufficiently will be touched upon, such as the problems of Generalisation ; of Essentialism ; of the rôle played by Intuitive Understanding ; of the Inexactitude of Prediction ; of Complexity ; and of the application of Quantitative Methods.

I do not intend to assert that there are no differences whatever between the methods of the theoretical natural and social sciences; such differences clearly exist, even between the various natural sciences themselves, as well as between the various social sciences. (Consider, for example, the analyses of the market and of language.) But I agree with people like Comte and Mill (and many others, such as C. Menger) that the methods in the two fields are fundamentally the same, though these methods are, perhaps, not what they had in mind. The methods always amount to deductive causal explanation, prediction, and testing, as sketched in the foregoing section. This has sometimes been called the hypothetic-deductive method,[1] or more often the method of hypothesis, for it does not achieve absolute certainty for any of the scientific statements which it tests; rather, these statements always retain the character of tentative hypotheses, even though their character of tentativeness may cease to be obvious after they have passed a great number of severe tests.

Because of their tentative or provisional character, hypotheses were considered, by most students of method, as *provisional in the sense that they have ultimately to be replaced by proved theories* (or at least theories which can be proved to be highly probable in the sense of some calculus of probabilities). I believe that this view is mistaken and that it leads to a host of entirely unnecessary difficulties. But this problem[2] is of comparatively little moment here. What is important is the realisation that in science, we are always concerned with explanations, predictions, and tests, and that the method of testing hypotheses is in the main always the same (see the foregoing section): from the hypothesis to be tested—for example a universal law—together with some other statements which for this purpose are considered as unproblematic—for example, some initial conditions—we deduce some prognosis. This prognosis we then confront, whenever possible, with the results of experimental or other observations. Agreement is considered as confirming evidence, though not as final proof; clear disagreement is considered as refutation or falsification.

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[8] If we succeed in determining the complete or sufficient singular conditions *c* of a singular trend *t*, then we can formulate the universal law : "Whenever there are conditions of the kind *c* there will be a trend of the kind *t*". The idea of such a law is unobjectionable from the logical point of view ; but it is very different from Comte's and Mill's idea of a law of succession which, like an absolute trend, or a law of a mathematical series, characterises the general run of a whole series of events. Besides, how could we determine that our conditions are sufficient ? Or what amounts to the same thing : how could we *test* a law of the form indicated above ? (We must not forget that we are here discussing the position (*b*) of section 27, which involves the claim that the trend *can be tested*.) In order to test such a law we have to attempt to produce conditions under which it does not hold ; to this end we must try to show that the conditions of the kind *c* are insufficient, and that even in their presence, a trend like *t* does not always occur. A method like this (it is sketched in section 32) would be unobjectionable ; but it is inapplicable to the historicist absolute trends, since these are necessary and omnipresent concomitants of social life, and cannot be eliminated by any possible interference with social conditions. (We can see here again the "metaphysical" character of the belief in trends which are not specific, e.g., of general trends : the statements expressing such a belief cannot be tested ; see also the foregoing note.)

[1] Cp. V. Kraft, *Die Grundformen der wissenschaftlichen Methoden* (1925).

According to this analysis, there is not a great difference between explanation, prediction, and testing. The difference is not one of logical structure, but rather one of emphasis; it depends on what we consider as problematic and what as unproblematic. If we consider the prognosis as unproblematic and the initial conditions or some of the universal laws (or both) as problematic, then we speak of an *explanation*. If we consider the laws and initial conditions as unproblematic and use them merely for deducing the prognosis, in order to get in this way some new information, then we speak of a *prediction*. (This is the case in which we *apply* our scientific results.) And if we consider one of the premises, i.e., either a universal law or an initial condition as problematic, and the prognosis as decidable by experience, then we speak of a *test* of the problematic premise.

The result of tests is the *selection* of hypotheses which have stood up to tests, by way of the *elimination* of those hypotheses which have not stood up to them, and which were therefore rejected. It is important to realise the consequences of this view. They are these: all tests can be interpreted as attempts to weed out false theories—to find the weak points of a theory in order to reject it if it is falsified by the test. This view is sometimes considered as paradoxical; our aim, it is said, is to establish theories, not to eliminate false ones. But just because it is our aim to establish theories as well as we can, we must test them as severely as we can; that is, we must try to find fault with them, we must try to falsify them. Only if we cannot falsify them in spite of our best efforts can we say that they have stood up to severe tests. This is the reason why the discovery of instances which confirm a theory means very little if we have not tried, and failed, to discover refutations. For if we are uncritical we shall always find what we want: we shall look for, and find, confirmations, and we shall look away from, and not see, whatever might be dangerous to our pet theories. In this way it is only too easy to obtain what appears to be overwhelming evidence in favour of a theory which, if approached critically, would have been refuted. In order to make the method of selection by elimination work, and to ensure that only the fittest theories survive, their struggle for life must be made severe.

This is, in outline, the method of all sciences which are backed by experience.

But what about the method by which we *obtain* our theories or hypotheses? What about *inductive generalisations*, and the way in which we proceed from observation to theory? To this question (and to the claims of section 1, so far as they have not been dealt with in section 26) I shall give two answers. (*a*) I do not believe that we ever make inductive generalisations in the sense that we start with observations and try to derive our theories from them. I believe that the prejudice that we proceed in this way is a kind of optical illusion, and that at no stage of scientific development do we begin without something in the nature of a theory, such as a hypothesis, or a prejudice, or a problem—often a technological one—which in some other way *guides* our observations,[3] and helps us to select from the innumerable objects of observation those which may be of interest. But if this is so, then the method of elimination—which is nothing but that of trial and error discussed in section 24—can be always applied. However, I do not think that it is necessary for our discussion to insist upon this point. For we can say (*b*) that it is irrelevant from the point of view of science whether we have obtained our theories by jumping to and stumbling over them, i.e., by " intuition ", or by some inductive procedure. The question: " how did you first *find* your theory ? " is so to speak of an entirely private nature, as opposed to the question " how did you *test* your theory ?" which alone is scientifically relevant. And the method of testing described here is fertile; it leads to new observations, and to a mutual give and take between theory and observation.

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[2] With this, cp. my *Logik der Forschung*, on which the present section is based, esp. the doctrine of tests by way of deduction (" deductivism "), and of the redundancy of any further " induction ", since theories always retain their hypothetic character (" hypotheticism "), and the doctrine that scientific tests are attempts at falsifying theories (" eliminationism "); see also the discussion of testability and falsifiability.

[3] For an interesting example of the way in which even botanical observations are guided by theory (and in which they may be even influenced by prejudice), cp. O. Frankel, " Cytology and Texonomy of Hebe, etc.", in *Nature*, Vol. 147 (1941), p. 117.

Now all this, I believe, is not only true for the natural, but also for the social sciences. And in the social sciences it is even more obvious than in the natural sciences that we cannot see and observe our objects before we have thought about them. For most of the objects of social science, if not all of them, are abstract objects; they are *theoretical* constructions. (Even " the war ", or " the army " are abstract concepts, strange as this may sound to some. What is concrete are the many who are killed; or the people in uniform, etc.) These objects are the result of constructing certain *models* (institutions), in order to explain certain experiences—a familiar theoretical method in the natural sciences (where we construct models of atoms, molecules, solids, liquids, etc.), and part of the method of explanation by way of reduction. That we are, very often, unaware of the fact that we are operating with theories, and that we mistake our theoretical models for things, is true, but this is a kind of mistake which is only too common.[4] This use of models explains and at the same time destroys the claims of methodological essentialism (cp. section 10). It explains them, for the model is of an abstract or theoretical character, and we are liable to believe that we see it, either within or behind the changing observable events, as a kind of permanent ghost or essence. And it destroys them because our task is to analyse our sociological models carefully in descriptive or nominalist terms, viz., *in terms of individuals*, their attitudes, expectations, relations, etc.—a postulate which may be called " methodological individualism ".

The unity of the methods of the natural and social sciences may be illustrated and defended by an analysis of two passages from Professor Hayek's *Scientism and the Study of Society*.[5] In the first passage, Professor Hayek writes:

> " The physicist who wishes to understand the problems of the social sciences with the help of an analogy from his own field would have to imagine a world in which he knew by direct observation the inside of the atoms and had neither the possibility of making experiments with lumps of matter nor opportunity to observe more than the interactions of a comparatively few atoms during a limited period. From his knowledge of the different kinds of atoms he could build up models of all the various ways in which they could combine into larger units and make these models more and more closely reproduce all the features of the few instances in which he was able to observe more complex phenomena. But the laws of the macrocosm which he could derive from his knowledge of the microcosm would always remain ' *deductive* '; they would, because of his limited knowledge of the data of the complex situation, scarcely ever enable him to predict the precise outcome of a particular situation; and he could never verify them by controlled experiment —although they might be *disproved* by the observation of events which according to his theory are impossible."

I admit that the first sentence of this passage points to certain differences between social and physical science. But the rest of the passage, I believe, speaks for a complete *unity of method.* For if it is, which I do not doubt, a correct description of the method of social science, then it shows that it differs only from such interpretations of the method of natural science as we have already rejected. I have in mind, more especially, the " inductivist " interpretation which holds that in the natural sciences we proceed systematically from observation to theory by some method of generalisation, and that we can establish, or perhaps even prove, our theories by some methods of induction, etc. A very different view has been advocated in the present section—an interpretation of scientific method as deductive, hypothetical, selective by way of falsification, etc. This description of the method of natural science agrees perfectly with Professor Hayek's description of the method of social science. (I have every reason to believe that my interpretation does describe the method of the natural sciences; for when I developed it, I had only the natural sciences in mind,[6] and disregarded the social sciences altogether.)

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[4] With this and the following paragraph, cp. F. A. v. Hayek, " Scientism and the Study of Society ", Parts I and II, ECONOMICA, Vols. IX and X, where methodological collectivism is criticised and where methodological individualism is discussed in detail.

[5] For the two passages see ECONOMICA, Vol. IX, p. 289f. (italics mine).

But even the differences alluded to in the first sentence of the quotation are not so great as may appear at first sight. It is undoubtedly true that we have a more direct knowledge of the " inside of the human atom " than we have of physical atoms; but this knowledge is intuitive. In other words, we certainly use our knowledge of ourselves in order to frame *hypotheses* about some other people, or about all people. But these hypotheses must be tested, they must be submitted to the method of selection by elimination. (By their intuition, some people are prevented from even imagining that anybody can possibly dislike chocolate.) The physicist, it is true, is not helped by such direct observation when he frames his hypotheses about atoms; nevertheless, he quite often uses some kind of sympathetic imagination or intuition which may easily make him feel that he is intimately acquainted with even the " inside of the atoms "—with even their whims and prejudices. But this intuition is his private affair. Science is interested only in the hypothesis which his intuitions may have inspired, and only if it is rich in consequences and can be properly tested. (These brief remarks may serve, at the same time, as an indication of the direction which our criticism of the claim discussed in section 8 would take, i.e., of the claim that the social science must use the method of intuitive understanding.—For the other difference mentioned in Professor Hayek's first sentence, i.e., the difficulty of conducting experiments, see section 24.)

> In the second passage, speaking of social phenomena, Professor Hayek says: ". . . our knowledge of the principle by which these phenomena are produced will rarely if ever enable us to predict the precise result of any *concrete* situation. While we can explain the principle on which certain phenomena are produced and can from this knowledge *exclude the possibility of certain results*, e.g., of certain events occurring together, our knowledge will in a sense be only negative, i.e., it will merely enable us to preclude certain results but not enable us to narrow the range of possibilities sufficiently so that only one remains ".

This passage, far from describing a situation peculiar to the social sciences, is a perfect description of the character of natural laws which, indeed, can never do more than *exclude certain possibilities*. (" You cannot carry water in a sieve "; cp. section 20, above.) The remark, more especially, that we shall not, as a rule, be able " to predict the precise result of any *concrete* situation " opens up the problem of the inexactitude of prediction (see section 5, above). I contend that precisely the same may be said of the concrete physical world. In general it is only by the use of artificial experimental isolation that we can predict physical events. (The solar system is a special case of such isolation: once its isolation is destroyed by the intrusion of a foreign body of sufficient size, all our predictions are liable to break down.) We are very far from being able to predict, even in physics, the precise results of a *concrete* situation, such as a thunderstorm, or a fire.

A very brief remark may be added here on the problem of complexity (cp. section 4, above). There is no doubt that the analysis of any concrete social situation is made extremely difficult by its complexity. But the same holds for any concrete physical situation.[7] The widely held prejudice that social situations are more complex than physical ones seems to arise from two sources. One of them is that we are liable to compare what should not be compared, viz., on the one side concrete social situations and on the other side artificial, i.e., experimental physical situations. (The latter might be compared, rather, with an " artificial social situation " such as a drill class, or a prison.) The other source is the old belief that the description of a social situation should involve the mental and perhaps even physical states of all persons concerned (or perhaps that it should even be reducible to them). But this belief is not justified; it is even much less justified than the impossible demand that the description of a concrete chemical reaction should involve that of the atomic and sub-atomic states of all elementary particles involved (although chemistry may indeed be reducible to physics); and it shows traces of the popular view that social entities such as institutions or associations are concrete natural entities such as crowds of men, rather than abstract models constructed to intrepret certain selected abstract relations between individuals.

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[6] Cp. *Erkenntnis III*, p. 426f., and my *Logik der Forschung* whose sub-title may be translated : " On the Epistemology of the Natural Sciences ".

[7] A somewhat similar argument can be found in C. Menger, *Collected Works*, Vol. II (1883 and 1933), pp. 259–260.

But, as a matter of fact, there are grounds not only for the belief that social science is less complicated than physics but also for the belief that concrete social situations are in general less complicated than concrete physical situations. For in most, if not in all, social situations there is an element of *rationality*. Admittedly, human beings hardly ever act quite rationally (i.e., as they would if they could make the optimal use of all available information for the attainment of whatever ends they may have), but they act, none the less, more or less approximately rationally; and this makes it possible to construct comparatively simple models of their actions and interactions, and to use these models as approximations.

The last point seems to me, indeed, to indicate a considerable difference between the natural and the social sciences—perhaps the *most important difference in their methods*, since the other important differences, i.e., the specific difficulties in conducting experiments (see end of section 24) and those of applying quantitative methods (see below) are differences of degree rather than of kind. I refer to the possibility of adopting, in the social sciences, what may be called the method of logical or rational construction, or perhaps the " zero method ".[8] By this I mean the method of constructing a model on the assumption of complete rationality (and perhaps also on the assumption of the complete possession of information) on the part of all individuals concerned, and of estimating the deviation of the actual behaviour of people from the model behaviour, using the latter as a kind of zero co-ordinate.[9] An example of this method is the comparison between actual behaviour (under the influence of, say, traditional prejudice, etc.) and model behaviour to be expected on the basis of the " pure logic of choice ", as described by the equations of economics. Marschak's interesting *Money Illusion*, for example, may be interpreted in this way.[10] An attempt at applying the zero method to a different field may be found in P. Sargant Florence's comparison[11] between the " logic of large scale operation " in industry and the " illogic of actual operation ".

I should like to mention, in passing, that I consider neither the principle of methodological individualism, nor that of the zero method of constructing rational models, as implying the adoption of a psychological method. On the contrary, I believe that these principles can be combined with the view[12] that the social sciences are comparatively independent of psychological assumptions, and that psychology can be treated, not as the basis of all social sciences, but as one social science among the others.

In concluding this section, I have to mention what I consider to be the other main difference between the methods of some of the natural and social sciences, viz., the specific difficulties connected with the application of quantitative methods, and especially methods of measurement.[13] Some of these difficulties can be, and have been, overcome by the application of statistical methods (as opposed to the claims of section 9), for example in demand analysis. And they *have to be overcome* if, for example, some of the equations of mathematical economics are to provide a basis even of merely qualitative applications; for without such measurement we should often not know whether or not some counteracting influences exceeded a merely qualitatively calculated effect. Thus merely qualitative considerations may be deceptive; just as deceptive (to quote Professor Frisch[14]) " as to say that when a man tries to row a boat forward, the boat will be driven backward because of the pressure exerted by his feet ". But it cannot be doubted that there are some fundamental difficulties here. In physics, for example, the parameters of our equations can, in principle, be reduced to a small number of natural constants—a reduction which has been successfully carried out in many important cases. This is not so in economics, where we even know that in the most important cases our parameters are themselves quickly changing variables.[15] This clearly reduces the significance, interpretation, and testability of measurements.

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[8] Cp. the " null hypothesis " discussed in J. Marschak, " Money Illusion and Demand Analysis ", in *The Review of Economic Statistics*, Vol. XXV, p. 40.—The method described here seems partly to coincide with what has been called by Professor Hayek, following C. Menger, the " compositive " method.

[9] Even here it may be said, perhaps, that the use of rational or " logical " models in the social sciences, or that of the " zero method ", has some vague parallelism in the natural sciences, esp. in thermodynamics and in biology (the construction of physiological models of organs and processes).

[10] Cp. J. Marschak, *op. cit.*

[11] Cp. P. Sargant Florence, *The Logic of Industrial Organisation* (1933).

[12] This view is more fully developed in Chapter 14 of my *Open Society, etc.*

[13] These difficulties are discussed by Professor Hayek, *op. cit.*, p. 290f.

[14] Cp. *Econometrica I* (1933), p. 1f.

[15] Cp. L. Robbins, in ECONOMICA, Vol. V, esp. p. 351.

(30) *Theoretical and Historical Sciences.* The thesis of the unity of scientific method, defended in the foregoing section so far as the theoretical sciences are concerned, can be extended, with certain limitations, even to the field of the historical sciences. And this can be done without giving up the fundamental distinction between theoretical and historical sciences—for example, between sociology or economic theory or political theory on the one hand, and social, economic, and political history on the other—which has been so often and emphatically re-affirmed by the best historians. It is the distinction between the interest in universal laws and the interest in particular facts. We adopt the view, so often attacked as old-fashioned by historicists, that *history is characterised by its interest in the actual, singular, or specific events, rather than in laws or generalisations.*

This view is perfectly compatible with the analysis of scientific method and especially of causal explanation in the preceding sections. The situation is simply this. While the theoretical sciences are mainly interested in finding and testing universal laws, the historical sciences take all kinds of universal laws for granted and are interested exclusively in finding and testing singular statements. Thus they will, for example, seek—given a certain singular " prognosis "—singular initial conditions which (together with all kinds of universal laws of little interest) explain the given prognosis. Or they will *test* a given singular hypothesis by using it, along with other singular statements, as an initial condition, and by deducing from these initial conditions (again with the help of all kinds of universal laws of little interest) some new prognosis which can be confronted with experience—perhaps with documents or inscriptions, etc.

In the sense of this analysis, *all* causal explanation of a singular event can be said to be historical in so far as the " cause " is always described by singular initial conditions. And this agrees entirely with the popular idea that to explain a thing causally is to explain how and why it happened, that is to say, to tell its " story ". But it is only in history that we are really interested in the causal explanation of a *singular* event. In the theoretical sciences, such causal explanations are merely means to a different end—the testing of universal laws.

If these considerations are correct, then the burning interest in questions of origin shown by some evolutionists and historicists, who despise the old-fashioned history and wish to reform it into a theoretical science, is somewhat misplaced. *Questions of origin are " how and why " questions. They are comparatively unimportant theoretically* and usually have only a specific historical interest.

Against our analysis of historical explanation[1] it may be argued that it *does* make use of universal laws, contrary to the emphatic declaration of so many historians that history has no interest whatever in such laws. To this we may answer that a singular event is the cause of another singular event—which is its effect—only relative to some universal laws.[2] But these laws may be so trivial, so much part of our common knowledge, that we need not mention them and rarely notice them. If we say that the cause of the death of Giordano Bruno was burning at the stake, we do not need to mention the universal law that all living things die when exposed to intense heat. But such a law was tacitly assumed in our causal explanation.

Among the theories which the political historian presupposes are, of course, certain theories of sociology—the sociology of power, for example. But the historian uses even these theories, as a rule, without being aware of them. He uses them, in the main, not as universal laws which help him to test his specific hypotheses but as implicit in his terminology: in speaking of governments, nations, armies, he uses, as a rule unconsciously, the " models " provided by scientific or pre-scientific sociological analysis (see the foregoing section).

The historical sciences, it may be remarked, do not stand quite apart in their attitude towards universal laws. Whenever we encounter an actual application of science to a singular or specific problem we find a similar situation. The practical chemist, for example, who wishes to analyse a certain given compound—a piece of rock, say—hardly considers any universal law. Instead, he applies, possibly without much thought, certain routine techniques which, from the logical point of view, are tests of such *singular* hypotheses as " this compound contains sulphur ". His interest is mainly at least a historical one—the description of one set of specific events, or of one individual body.

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[1] Our analysis may be contrasted with that of Morton G. White, " Historical Explanation " (*Mind*, N.S., Vol. 52, pp. 212ff.) who bases his analysis on my theory of causal explanation as reproduced in an article by C. G. Hempel. Nevertheless he reaches a very different result. Neglecting history's characteristic interest in singular sentences, he suggests that an explanation is " historical " if it is characterised by the use of *sociological terms* (and theories).

[2] This has been seen by Max Weber. His remarks on p. 179 of his *Ges. Schr. zur Wissenschaftslehre* (1922) are perhaps closer than any other I know to the analysis offered here. But he is mistaken, I believe, when he suggests that the difference between theoretical and historical science lies in the degree of generality of the laws used.

Our analysis clarifies, I believe, some well known controversies between certain students of the method of history.[3] One historicist group asserts that history, which does not merely enumerate facts but attempts to present them in some kind of causal connection, must be interested in the formulation of historical laws, since causality means, fundamentally, determination by law. Another group, which also includes historicists, argues that even " unique " events, events which occur only once and have nothing " general " about them, may be the causes of other events, and that it is this kind of causation that history is interested in. We can now see that both groups were partly right and partly wrong. Universal laws and specific events are together necessary for any causal explanation, but outside the theoretical sciences, universal laws usually arouse little interest.

This leads us to the question of the *uniqueness* of historical events. In so far as we are concerned with the historical explanation of typical events they must necessarily be treated as typical, as belonging to kinds or classes of events. For only then is the deductive method of causal explanation applicable. History, however, is interested not only in the explanation of specific events but also in the description of a specific event as such. One of its most important tasks is undoubtedly to describe interesting happenings in all their peculiarity or uniqueness; that is to say, to include aspects which it does not attempt to explain causally, such as the " accidental " concurrence of causally unrelated events. These two tasks of history, the disentanglement of causal threads and the description of the " accidental " manner in which these threads are interwoven are both necessary, and both supplement each other; at one time an event may be considered as typical, i.e., from the standpoint of its causal explanation, and at another time as unique.

These considerations may be applied to the question of *novelty*, discussed in section 3. The distinction made there between " novelty of arrangement " and " intrinsic newness " corresponds to the present distinction between the standpoint of causal explanation and that of the appreciation of the unique. So far as newness can be rationally analysed and predicted, it can never be " intrinsic ". This dispels the historicist claim that social science should be applicable to the problem of predicting the emergence of intrinsically new events—a claim which may be said to rest ultimately on an insufficient analysis of prediction and of causal explanation.

(31) *Situational Logic in History. Historical Interpretation.* But is this all? Is there nothing whatever in the historicist demand for a reform of history—for a sociology which plays the rôle of a theoretical history, or a theory of historical development? (Cp. sections 12 and 16.) Is there nothing whatever in the historicist idea of " periods "; of the " spirit " or " style " of an age; of irresistible historical tendencies; of movements which captivate the minds of individuals and which surge on like a flood, driving, rather than being driven by, individual men? Nobody who has read, for example, the undoubtedly historicist but candid and well motivated speculations of Tolstoy, in *War and Peace*, on the movement of the men of the West towards the East and the counter movement of the Russians towards the West, can deny that historicism answers a real need. We have to satisfy this need by offering something better before we can seriously hope to get rid of historicism.

Tolstoy's historicism is a reaction against the method of " hegemonism " in history, i.e., the method that attributes much—too much, if Tolstoy is right, as he undoubtedly is—to the great man, the leader. Tolstoy tries to show, successfully I think, of what little influence were the actions and decisions of Napoleon, Alexander, Kutuzov, and the other great leaders of 1812, in the face of what may be called the logic of events. Tolstoy points out, rightly, the neglected but very great importance of the decisions and actions of the countless unknown individuals who fought the battles, who burned Moscow, and who invented the partisan method of fighting. But he believes that he can see some kind of historical determination in these events—fate, historical laws, or a plan. His historicism combines a radical methodological individualism with a strong collectivism; that is to say, he represents a rather typical combination of democratic-individualist and collectivist-nationalistic elements.

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[3] See, for example, Weber, *op. cit.*, pp. 8f., 44f., 48, 215ff., 233ff.

This example may remind us that there are *some* sound elements in historicism; it is a reaction against a naïve hegemonism, against the method of interpreting political history merely as the story of great tyrants and of great generals. Historicism rightly feels that there may be something better than this method. It is this feeling which makes the idea of " spirits "—of an age, of a nation, of an army—so popular.

Now I have not the slightest sympathy with these " spirits " ; neither with their idealistic prototype nor with their dialectical and materialistic incarnations, and I am in full sympathy with those who treat them with contempt. And yet, I feel that they indicate, at least, the existence of a vacuum, of a place which it is the task of sociology to fill with something more sensible. There is room for a more detailed analysis of the *logic of situations* of which the best historians have often made use more or less unconsciously—for example Tolstoy, when he describes how it was not decision but " necessity " which made the Russian army yield Moscow without a fight and withdraw to places where it could find food. Beyond this logic of situations, or perhaps as a part of it, we need something like an analysis of social movements. We need studies, based on methodological individualism, of the social institutions through which ideas may spread and captivate individuals, of the way in which new traditions may be created, and of the way in which traditions work and break down. In other words, our individualistic and institutionalist models of such collective entities as nations, or governments, or markets, will have to be supplemented with models of political situations as well as of social movements such as scientific and industrial progress. (A sketch of such an analysis of progress will be found in the next section.) These models may then be used by historians, partly like the other models, and partly for the purpose of explanation, among the other universal laws they use. But even this would not be enough; it would not answer all these real needs which historicism answers.

If we consider the historical sciences in the light of our comparison between them and the theoretical sciences, then we can see that their lack of interest in universal laws puts them in a difficult position. For in theoretical science universal laws act among other things as centres of interest to which observations are related, or as points of view from which observations are made. In history, the mostly unconsciously used and trivial universal laws cannot possibly fulfil this function. It must be taken over by something else. For undoubtedly there can be no history without a point of view; like the natural sciences, history must be *selective* unless it is to be choked by a flood of poor and unrelated material. The attempt to follow causal chains into the remote past would not help in the least, for every concrete effect with which we might start has a great number of indifferent partial causes; that is to say, initial conditions are very complex, and the greater part of them have no interest.

The only way out of this difficulty is, I believe, consciously to introduce a *preconceived selective point of view* into history, that is to say, to write *that history which interests us*. This does not mean that we may twist the facts until they fit into a framework of preconceived ideas, or that we may neglect the facts that do not fit.[1] On the contrary, all available evidence which has a bearing on our point of view should be considered carefully and objectively (in the sense of " scientific objectivity " as discussed in the next section). But it means that we need not worry about all those facts and aspects which have no bearing upon our point of view and which therefore do not interest us.

Such selective points of view fulfil functions in history which are in many ways analogous to those of theories in science. It is therefore understandable that they have been often taken for theories. And indeed, those points of view which can be formulated in the form of *testable hypotheses*, whether singular or universal, may well be treated as scientific hypotheses. But as a rule, historial points of view *cannot be tested*. They cannot be refuted, and apparent confirmations are therefore of no value. We shall call such a selective point of view or focus of historical interest, if it cannot be formulated as a testable hypothesis, a *historical interpretation*.

Historicism mistakes interpretations for theories. This is one of its cardinal errors. It is possible, for example, to interpret " history " as the history of class struggle, or as the struggle of races for supremacy, or as the history of religious ideas, or as the history of the struggle between the " Open " and the " Closed " society, or as the history of scientific and industrial progress. All these are more or less interesting points of view, and *as such* perfectly unobjectionable. But historicists do not present them as such; they do not see that there is necessarily a plurality of interpretations which are fundamentally equivalent (even though some of them may be distinguished by their *fertility*—a rather important point). Instead, they present them as doctrines or theories, asserting that " all history is the history of class struggle ", etc. And if they actually find that their point of view is fertile, and that many facts can be ordered and interpreted in its light, then they mistake this as a confirmation, or preferably as a proof, of their doctrine.

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[1] For a criticism of the "doctrine . . . that all historical knowledge is relative", see Hayek, in ECONOMICA, Vol. X, pp. 55ff.

The classical historians who rightly oppose this procedure, on the other hand, are liable to fall into further error. Aiming at objectivity, they feel bound to avoid any selective point of view; but since this is impossible, they usually adopt points of view without being aware of them. This must defeat their efforts to be objective, for one cannot possibly be critical of anything, and conscious of its limitations, without being aware of it.

The way out of this dilemma, of course, is to be clear about the necessity of adopting a point of view; to state it plainly, and always to remain conscious that it is one among many, and that it is not testable.

(32) *The Institutional Theory of Progress.* In order to make our considerations less abstract, I shall try in this section to sketch, in very brief outline, a *theory of scientific and industrial progress* on the principles indicated in sections 28, 29, and 31. Scientific and industrial progress is chosen because this phenomenon undoubtedly furnished the inspiring intuition of modern (i.e., 19th century) historicism, and because we have previously discussed some of Mill's views on this subject.

The theory of progress developed by Comte and Mill, it will be remembered, is one of an unconditional or absolute trend, which is *reducible to the laws of human nature*. " A law of succession," writes Comte,[1] " even when indicated with all possible authority by the method of historical observation, ought not to be finally admitted before it has been rationally reduced to the positive theory of human nature . . ." He believes that the law of progress is deducible from a tendency in human individuals which impels them to perfect their nature more and more. In all this, Mill follows him completely, trying to reduce his law of progress to what he calls the " progressiveness of the human mind "[2] whose first " impelling force . . . is the desire of increased material comforts ". According to both Comte and Mill the unconditional or absolute character of this trend or quasi-law enables us to deduce from it the first steps or phases of history, without requiring any initial historical conditions or observations or data.[3] In principle, the whole course of history should be thus deducible; the only difficulty being, as Mill puts it,[4] that " so long a series . . ., each successive term being composed of an even greater number and variety of parts, could not possibly be computed by human faculties."

The weakness of this reduction seems obvious. Even if Mill's premises and deduction are granted, it still does not follow that the social or historical effect will be significant. Progress may be rendered negligible by systematic losses due to an unmanageable natural environment. Besides, the premises are based on only one side of " human nature " without considering other sides such as forgetfulness or indolence. Thus where we observe the precise opposite of the progress described by Mill, there we can equally well " reduce " these observations to " human nature ". (Is it not, indeed, one of the most popular devices of so-called historical theories to explain the decline and fall of empires by such traits as idleness and a propensity to overfeed?) In fact we can conceive of very few events which could not be plausibly explained by " human nature ". But a method that explains everything that might happen explains nothing.

If we wish to replace this amazingly naïve theory by a more tenable one, we have to make two main alterations. First, we have to attempt to find *conditions* of progress, and to this end we must try to imagine, for example, conditions under which progress would be arrested. This immediately leads to the realisation that it can hardly be a psychological propensity which impels man to progress, since no conditions could then be found to arrest it. Thus we must next replace the theory of psychological propensities by something better; I suggest, by an *institutional* (and technological) analysis.

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[1] Comte, *Cours de philosophie positive*, IV, p. 335.

[2] Mill, *Logic*, Book VI, Ch. X, section 3; the next quotation is from section 6 where the theory is worked out in more detail.

[3] Comte, *op. cit.*, IV, p. 345.

[4] Mill, *loc. cit.*, section 4.

How could we arrest scientific and industrial progress? By closing down or controlling laboratories for research, by suppressing or controlling scientific periodicals and other means of discussion, by suppressing scientific congresses and conferences, by suppressing Universities and other schools, by suppressing books, the printing press, writing, and, in the end, speaking. All these things which are to be suppressed (or controlled) are social institutions. Language is a social institution without which scientific progress is hardly thinkable, since without it there can be neither science nor a growing and progressive tradition. Writing is a social institution, and so are the organisations for printing and publishing and all the other institutional instruments of scientific method. Scientific method itself has social aspects. Science, and more especially scientific progress, are the results not of isolated efforts but of the *free competition of thought*. For science needs ever more competition between hypotheses and ever more rigorous tests, and the competing hypotheses need personal representation, as it were: they need advocates, they need a jury, and even a public. This personal representation must be institutionally organised if we wish to ensure that it works. And these institutions have to be paid for, and protected by law. Ultimately, progress depends very largely on political factors; on political institutions that safeguard the freedom of thought: on democracy.

It is of some interest that what is usually called " *scientific objectivity* " is based, to some extent, on social institutions. The naïve view that scientific objectivity rests on the mental or psychological attitude of the individual scientist, on his training, care, and scientific aloofness, produces as a reaction the sceptical view that scientists can never be objective. On this view their lack of objectivity may be negligible in the natural sciences where their passions are not excited, but in the social sciences it becomes a major fatality, owing to social prejudices, class bias and similar unavoidable influences. This doctrine, developed in detail by the so-called " sociology of knowledge " (cp. sections 6 and 26) entirely overlooks the social or institutional character of scientific knowledge, being still based on the naïve view that objectivity depends on the psychology of the individual scientist. It does not see that neither the dryness nor the remoteness of a topic of natural science prevent partiality and self-interest from interfering with the individual scientist's beliefs, and that if we had to depend on his aloofness, science, even natural science, would be quite impossible. What the " sociology of knowledge " overlooks is just the sociology of knowledge, i.e., the social or public character of science; it overlooks that its inter-subjectivity and its institutions for dissemination and discussion are the safeguards of scientific objectivity. They also impose a mental discipline upon the individual scientist. In this connection, we may briefly comment on another of the claims presented in section 6 (*Objectivity and Valuation*). There it was claimed that, since scientific research in social problems must itself influence social life, it is impossible for the social scientist who is aware of this influence to retain the proper scientific attitude of disinterested objectivity. But there is nothing peculiar to social science in this situation. A physicist or a physical engineer is in the same position. Without being a social scientist he can realise that the invention of a new aircraft may have a tremendous influence on society.

We have just sketched some of the institutional conditions on whose realisation scientific and industrial progress depends. It is now important to realise that most of these conditions cannot be called necessary; and that all of them taken together are not sufficient.

The conditions are not necessary, since without the institutions described, language excepted, scientific progress would not be strictly impossible. " Progress ", after all, *has* been made from the spoken to the written word, and even further (although this early development was perhaps not, strictly speaking, scientific progress).

On the other hand, and this is more important, we must realise that with the best institutional organisation in the world, scientific progress may one day stop. There may, for example, be an epidemic of mysticism. This is certainly possible, for since some intellectualists *do* react to scientific progress by withdrawing into mysticism, everyone *might* react in this way. Such a possibility may perhaps be counteracted by devising a further set of social institutions, such as educational institutions, to discourage uniformity of outlook and encourage diversity. Also, the idea of progress and its enthusiastic propagation may have some effect. But all this cannot make progress certain. For we cannot exclude the logical possibility, say, of a bacteria or virus that spreads a wish for the Nirvana. We thus find that even the best institutions can never be foolproof; as we said in section 21: institutions are like fortresses. They are effective only if they are properly manned.

I believe that this analysis is typical, and that the human or personal factor must be generally *the* irrational element in most, or all, institutional social theories. The opposite doctrine which teaches the reduction of social theories to psychology, in a similar way as we try to reduce chemistry to physics, is, I believe, based on a misunderstanding. It arises from the false belief that this " methodological psychologism " is a necessary corollary of a methodological individualism—of the quite unassailable doctrine that we must reduce all collective phenomena to the actions, interactions, aims, hopes, and thoughts, of individuals. But we can be individualists without accepting psychologism. The " zero method " of constructing rational models is *not* a psychological but rather a logical method.

With this, we reach a result which strangely contrasts with the still fashionable method of Comte and Mill. Instead of reducing sociological considerations to the apparently firm basis of the psychology of human nature, we find that the human factor is *the* ultimately uncertain and irrational element in our theory. This is the element which ultimately *cannot* be completely controlled by institutions (as Spinoza first saw[5]); for every attempt at controlling it completely must lead to tyranny, and with it, to the omnipotence of the human factor—of the whim of one man, or of a few men.

But is it not possible to control the human factor by *science*—the opposite of whim? In theory biology and psychology probably can solve, or soon will be able to solve, the " problem of transforming man " (cp. section 21). Yet if they attempt this in practice, they must destroy the objectivity of science, and so science itself, since these are both based upon free competition of thought, i.e., upon freedom. If the growth of reason is to continue, and human rationality to survive, then the diversity of individuals and their opinions, aims, and purposes must never be interfered with (except in extreme cases where political freedom is endangered). Even the emotionally satisfying appeal for a *common purpose*, however excellent, is an appeal for the end of rational thought.

The evolutionist who demands the " scientific " control of human nature does not realise how suicidal this demand is. The mainspring of evolution and progress is the variety of material to undergo selection. So far as human evolution is concerned it is the " freedom to be odd and unlike one's neighbour ",—" to disagree with the majority, and go one's own way ".[6] Holistic control, that is the equalisation not of human rights but of human minds, would mean the end of progress.

(33) *Conclusion. The Emotional Appeal of Historicism.* Historicism is a very old movement. Its oldest forms, such as the doctrines of the life cycles of cities and races, actually precede the primitive teleological view that there are hidden purposes[1] behind the apparently blind decrees of fate. Although this divination of hidden purposes is far removed from the scientific way of thinking it has left unmistakable traces upon even the most modern historicist theories: every version of historicism expresses the feeling of being swept into the future by irresistible forces.

Modern historicists, however, are seemingly unaware of the antiquity of their doctrine. They believe—and what else could their deification of modernism permit? —that their own brand of historicism is the latest and boldest achievement of the human mind, an achievement so staggeringly novel that only a few people are capable of grasping it. They believe, indeed, that it is they who have discovered the problem of change—one of the oldest problems of speculative metaphysics. Contrasting their " dynamic " thinking with the " static " thinking of all previous generations, they believe that their own advance has been made possible by the fact that we are living in a revolution which has so much accelerated the speed of our development that change can be directly experienced within a single lifetime. This story is, of course, sheer mythology. Important revolutions have occurred before our time, and change has been discovered over and over again.

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[5] Cp. note 5 to section 24, above.

[6] See Waddington (*The Scientific Attitude*, 1941, pp. 111 and 112) who is prevented neither by his evolutionism nor by his scientific ethics from denying that this freedom has a scientific value. Waddington's passage is criticised in Hayek's *The Road to Serfdom*, p. 143.

[1] The best immanent criticism of the teleological doctrine known to me (immanent in so far as it adopts the religious point of view and especially the doctrine of creation) is the last chapter of M. B. Foster's *The Political Philosophies of Plato and Hegel*.

It shows an unconscious conservatism to offer such a venerable idea as bold and revolutionary. But this emphasis on the novelty and boldness of the idea of change shows the inner resistance to be overcome, and so explains the emotionalism with which the newness of this antique philosophy is advocated. Such considerations might suggest that it is the historicists who are afraid of change, and that they could not accept the idea without serious inward struggles. And this, in turn, might explain their appeal as well as their peculiar dogmatism. It even seems as if the historicists are trying to comfort themselves for the loss of an unchanging world by clinging to the notion that change can be foreseen because it is ruled by an unchanging law.

Notes on Cost

By P. T. Bauer

A RECENT article[1] by the present writer discussed the plasticity of costs with reference to the experience of eight Malayan rubber companies during the years 1929–33. It was there suggested that the prosperity of the industry and the price of the commodity were much more important factors influencing costs than changes in the rate of production. In the next few pages this point will be developed by reference to a much greater sample, comprising the great bulk of British-owned rubber companies, operating under more diverse conditions than the eight companies previously analysed. This larger sample, moreover, reveals certain features of interest which were not disclosed by the previous investigation, and by which light is thrown on some more general aspects of supply and of cost of production, especially on user cost.

I

1. Until the spring of 1930, the majority of sterling rubber companies (companies registered in the United Kingdom) used to publish their f.o.b. and/or their all-in costs of production. These cost figures, particularly the latter, were not free from ambiguity; the varying proportions of total output sold on f.o.b., c.i.f., or landed terms affected freight and selling costs, while the treatment of amortisation and depreciation charges was not always consistent. In 1930 the Rubber Growers' Association (R.G.A.) recommended to its members to cease publishing their costs which were said to be misleading and, in particular, to understate true long-period costs owing to inadequate provision for capital charges. At about the same time the R.G.A. started a cost enquiry of its own and ever since 1929 its members have furnished monthly returns of their cost of production to the Commercial Research Department of the Association. In the early 1930's returns were received from about 330–350 companies (the number varied slightly from year to year) and covered over a million mature acres, some two-thirds of the mature estate area owned by companies domiciled in the U.K. The cost figures have always been treated as a closely guarded secret[2] though they were made available to the International Rubber Regulation Committee to whose records the writer has been given unrestricted access.

2. Table I shows sterling company costs as calculated by the Commercial Research Department of the R.G.A. from the monthly returns. The Table is largely self-explanatory, but the following points should be noted. To secure a large measure of comparability, amortisation costs and freight and selling charges have been omitted from all-in costs, though a small depreciation allowance is included; f.o.b. costs cover all cash costs to board ship. The costs of companies operating in the N.E.I. have been converted at the average rate of exchange ruling during the various periods referred to. The years chosen, 1929–33, cover the only period of unrestricted production since 1918.

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[1] *Economic Journal*, December, 1943.

[2] This secrecy has been both unnecessary and ineffective. The R.G.A. recommendation against the publication of costs was widely, but by no means universally, accepted and an examination of reference books disclosed that 64 sterling companies (out of a total of some 500) regularly published their costs between 1930 and 1934 and many more did so occasionally. These 64 companies were with few exceptions outside the membership of the leading agency and secretarial groups of the R.G.A. which followed the official recommendation. Until 1931 *The Economist* used to publish the costs of over 100 sterling companies which were practically all members of the leading secretarial groups. There was, for instance, an overlap of only two companies between the 103 whose costs were reviewed in *The Economist* of May 10th, 1930, and the 64 which published their costs throughout the depression; the former represented the larger and the latter the smaller companies and groups. Yet in 1929 their costs were almost identical; the arithmetic means and the modes of their f.o.b. and all-in costs were all within 2 per cent. It was thus clear from the start that the costs of the 64 companies would be representative of sterling company costs, and throughout the period under review their arithmetic mean approximated very closely to those calculated by the R.G.A. from the returns of its members. The requisite information was thus available to those prepared to go to some trouble. Quite recently the American authorities have made public some of the R.G.A. cost figures furnished to the I.R.R.C. between 1935 and 1941.

A sub-division of the costs summarised in Table I, showing the various constituent items and the relative decline under each heading, is given in Appendix II to this article, where the average costs of the companies are also shown according to the territories they were operating in. Table II summarises the costs for 1929–33 of 45 locally registered Malayan companies (dollar companies); the reference is to all-in cash costs of rubber delivered in Singapore. The figures have been extracted from various publications of Malayan stockbrokers. Fiscal years ending between 1st July, 1929, and 30th June, 1930, have been taken as 1929 and similarly for subsequent years.

The unweighted arithmetic mean is used in both Tables.

TABLE I

AVERAGE COSTS OF PRODUCTION, IN PENCE PER LB. OF RUBBER COMPANIES FURNISHING RETURNS TO THE RUBBER GROWERS' ASSOCIATION.

Financial year ending in	(*a*) All-in Costs		(*b*) F.o.b. Costs		(*c*) Average London price of rubber for corresponding financial year	
1929	Pence	*Index*	Pence	*Index*	Pence	*Index*
1st quarter ..	6·58	*100*	5·71	*100*	9·54	*100*
2nd ,, ..	6·31	*100*	5·57	*100*	9·94	*100*
3rd ,, ..	5·88	*100*	5·26	*100*	10·29	*100*
4th ,, ..	5·93	*100*	5·29	*100*	10·26	*100*
1930						
1st quarter ..	6·33	*96*	5·45	*97*	9·37	*98*
2nd ,, ..	6·51	*103*	5·83	*105*	8·41	*84*
3rd ,, ..	5·71	*97*	5·12	*98*	6·97	*68*
4th ,, ..	5·35	*90*	4·72	*89*	5·91	*58*
1931						
1st quarter ..	5·05	*77*	4·40	*77*	4·97	*52*
2nd ,, ..	4·41	*69*	3·84	*69*	4·06	*41*
3rd ,, ..	3·93	*67*	3·38	*64*	3·48	*34*
4th ,, ..	3·54	*59*	3·02	*57*	3·17	*31*
1932						
1st quarter ..	3·09	*47*	2·71	*47*	2·69	*28*
2nd ,, ..	3·02	*48*	2·61	*47*	2·38	*24*
3rd ,, ..	3·13	*53*	2·63	*50*	2·31	*22*
4th ,, ..	2·78	*47*	2·34	*44*	2·34	*23*
1933						
1st quarter ..	2·34	*36*	Not available		2·22	*23*
2nd ,, ..	2·52	*40*	,,		2·50	*25*
3rd ,, ..	2·89	*49*	,,		2·84	*28*
4th ,, ..	2·95	*50*	,,		3·25	*32*

N.B.—The index figures are expressed in percentages of costs for the fiscal year ending in the corresponding quarter of 1929.

TABLE II

AVERAGE ALL-IN CASH COSTS OF 45 DOLLAR COMPANIES, 1929–33: STRAITS CENTS PER LB.

	1929	1930	1931	1932	1933
Cash Cost	22·76	17·82	10·99	7·60	8·05
Index	*100*	*78*	*48*	*33*	*35*
Singapore price of rubber	34·48	19·31	9·96	7·01	10·23
Index	*100*	*56*	*29*	*20*	*30*

3. The figures given in the Tables serve to explain how it came about that while early in 1930 8d. was considered a poor or disastrous price, three years later company chairmen repeatedly stated that with a London price of 3d. or 3½d. their companies could make a reasonable profit. The extent of the fall in costs within three years is also well illustrated by the fact that among the dollar companies the all-in costs of the cheapest producer in 1929 were 60 per cent. above those of the highest cost producer in 1932.[1]

4. Table III of the *Economic Journal* article reviewed the steep fall in the wages of Indian labourers on the estates of the eight companies there analysed. It was stated that the mode of men's wages had declined from 50 to 28 cents a day between the end of 1929 and of 1932. Severe as was this reduction, it appears to have understated the general fall in the wages of South Indian estate labourers. The companies reviewed, though broadly representative in their technique of cultivation and their 1929 costs of production, were financially among the strongest in Malaya, and their wage and salary cuts below the general average.[2] More important still, wages had touched bottom in the early summer of 1932, and there was a definite rise between June and December, 1932. For various reasons, which cannot be dealt with here, estimates of the daily wages received by the general run of South Indian labourers during this period are subject to a small margin of error. The figures summarised in Table III are, however, reasonably close and may be regarded as the mode of the wages of South Indian estate workmen at the dates indicated.[3] The Singapore price of rubber (ribbed smoked sheet) at the corresponding dates is also given.

TABLE III

APPROXIMATE MODE OF DAILY WAGES (IN STRAITS CENTS) OF SOUTH INDIAN ESTATE WORKMEN ON MALAYAN PLANTATIONS, AND SINGAPORE PRICE OF RUBBER (IN STRAITS CENTS TO THE NEAREST INTEGRAL), 1929–33.

	End 1929	End 1930	End 1931	Mid 1932	End 1932	End 1933
Estate Wages ..	50	40	30	20–24	24–28	30–35
Price of rubber (per lb.)	26	13	10	5	7	14

Wages of Chinese estate labourers, who were employed chiefly on Chinese-owned properties, or by contractors carrying out special works such as felling, clearing or bud-grafting, fell proportionately much more. At the end of 1929 their daily earnings had been around 80–90 cents; by mid-1932 they were about level with Indian wages at around 20–25 cents. Chinese coolies and their employers frequently have a definite profit-sharing outlook on wage-earning and this was one, though not the only, reason for the specially violent fluctuations in the wages and earnings of Chinese coolies in Malaya.

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[1] Somewhat analogous to the plasticity of estate costs was the sharp reduction in the margin between the world price of rubber as measured by the Singapore, New York or London quotations, and the price paid for native rubber in the interior districts, especially in Borneo and Sumatra. The late Dr. Whitford estimated at 8 U.S. cents per lb. in 1929 the spread between the New York price of processed Dutch native rubber and the price paid in the interior of Sumatra and Borneo. For 1932 he thought the figure was around 2½ cents, possibly even 2 cents by the summer and autumn of that year: remarkably small margins, which included the remuneration of several middlemen, re-milling charges in Singapore, and transport cum delivery costs from the interior to New York via Singapore. Middlemen's charges and re-milling costs had fallen particularly heavily.

[2] The tendency for sharper wage and salary cuts by financially weaker companies was most noticeable, and especially so among locally registered enterprises, which were on the whole less strongly financed than the sterling companies; hence the steeper fall in the costs of the former. The weaker undertakings were forced and at the same time able, to cut salaries and wages more severely than the more prosperous enterprises. The Incorporated Society of Planters (the professional association of Malayan estate managers and assistants) was inclined to offer stiffer resistance to reductions by companies with ample reserves; the Labour Department was also less prepared to consent to near-starvation wages on estates which still had reserves, than where the alternative was closing down. Pressure by the authorities for prompt payment of rents was also greatest on companies with substantial cash reserves. It had been noticed early in the slump that the survival of many enterprises depended more on their financial resources than on their costs, and, according to a Malayan wit, the struggle was for the survival of the fattest, rather than for that of the fittest. The differential cuts by the financially weakest offset to an arbitrary degree their poorer life expectations.

[3] The wage-rates are cash payments, generally for morning work (which was the tapper's working day) only. They exclude various items in kind, such as free housing and medical attention.

5. The very sharp fall in costs during the slump cannot be attributed to any change in the scale of production. Throughout these years estate output in Malaya and Sumatra remained remarkably stable, whether expressed as total production or in terms of output per mature acre. For certain purposes the latter is relevant when comparing the scale of production over a period of years.[1]

The figures in Table IV refer to the total Malayan estate output, as recorded officially, and not to the output covered by the companies returning cost figures to the R.G.A. This simplifies the presentation of the data without affecting the argument. The R.G.A. figures are given in Appendix II.

TABLE IV

MALAYAN ESTATE PRODUCTION OF RUBBER, 1929–33

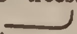
	1929	1930	1931	1932	1933
Total output—tons (nearest 1,000 tons) ..	246,000	237,000	240,000	240,000	241,000
Output per mature acre–lb. (nearest 5 lb.)	410	380	375	365	355

Throughout this period the mature area (which for any given year is taken as the total acreage planted up to five or more years previously) was increasing; the falling output per mature acre thus does not mean that the least efficient estates continued production, but that part of the mature area was left untapped.

The figures of the yield per mature acre are somewhat conjectural, owing to the uncertainty of the age-distribution of the planted area, especially in some of the Unfederated Malay States. The figures are based on the records of the International Rubber Regulation Committee, and are certain to indicate correctly the orders of magnitude.

II

6. In 1929, perfect competition prevailed in the rubber industry. No producer (whether single company, or secretarial group) controlled even as much as 2 per cent. of the annual output. During the year, the London price ranged between 8d. and 11d., while direct costs of production (direct f.o.b. costs plus freight and selling expenses) were generally below 4d. It may be asked how this was compatible with perfect competition, and why individual producers failed to expand output up to the point at which direct costs equalled price, in accordance with the marginal cost equals marginal revenue formula for maximising net revenue.

It might be argued that a similar relationship between price and prime costs is a common feature of agricultural production, and that the industry was working to capacity and no further explanation is required. Rubber differs, however, from all other forms of agriculture in that the annual output can be varied substantially with the rate of bark removal. Capacity output would have to be interpreted as the level of production at which the rate of bark removal equals the rate of renewal; harvesting in excess of this rate could be considered as dipping into capital. This does not dispose of the question, since a severer rate of tapping over a period of one or two years could be compensated by subsequent resting of the trees. Again, the marginal cost curve of output from most estates is shaped  (not unlike in manufacturing industry) and tapping beyond a certain intensity results in a sharp rise in prime costs, as the dry rubber content of latex tends to fall with too frequent tapping, while less skilled labour may have to be used; moreover very severe tapping systems may also injure the trees. It could be said that tapping at such rates is again in excess of long-period capacity. On a number of estates this point was reached, or at least approached in 1929, but on the great majority it certainly was not. Whatever definition of capacity working is adopted, a simple statement that production was at full capacity rate in 1929 does not explain the wide gap between price and direct costs.

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[1] See Appendix I at the end of this article.

7. Direct cost, as found in company accounts, is the marginal cash cost, and does not include the user cost element in bark removal. Tapping is the opening of the latex vessels in the tree, through incision in the bark. The flow of latex is generally less affected by the thickness of the cut than by its length. A skilled tapper will achieve satisfactory results by a very thin cut, but some bark removal is inevitable, and this contains an element of user cost however conservative the rate of tapping. If a tree is left untapped, bark reserves will be greater, to be used should a boom develop in the future, though in practice this consideration is not likely to be of great importance. Moreover, the number of latex vessels decreases with the height of the tree, particularly in seedling trees. The less frequently a tree is tapped, the lower it is possible to keep the tapping panel, with consequently higher yield per tree, and lower costs per lb.

The user cost element becomes much more important when a system of tapping is considered, under which the rate of bark removal would exceed bark renewal, since under such a system it would eventually be necessary to suspend or slow down tapping, or else to tap on imperfectly renewed bark. To maximise profits, the strictly rational course would be to vary the rate of tapping with the price. If the excess of price over direct costs is greater than it is likely to be when the tapping panel becomes tappable again at the ordinary rate of bark renewal, it would be quite rational to tap more severely as the additional gain now more than outweighs the subsequent loss due to the higher costs, or lower crop. Conversely, when present quasi-rents (prime profits) are thought lower than could be expected in the future, the rate of bark removal would be kept within the rate of renewal and in exceptional circumstances it would be worth-while to rest the trees altogether.[1]

8. In practice, such a system would normally be subject to severe limitations. Malaya, Ceylon, Sumatra and Borneo all depend on immigrant labour, and in 1929 and 1930 local labour markets were still in their infancy, and the supply of labour was usually not elastic enough for such rapid and delicate adjustments of output.[2] The extreme uncertainty of future prospects also acted as a limiting factor. To gauge costs and prices two or three years ahead is always hazardous, and more so in rubber than perhaps in any other industry.

Frequent adjustments in output to meet changes in market prices would have introduced considerable complexity and additional work in the management of the estates, and would have been greatly resented by the managers.[3] The somewhat contemptuous attitude of modern business executives towards the anonymous and inactive shareholders is well known, and rubber estate managers who live and work thousands of miles away from the particularly apathetic rubber shareholders, and who often form a distinct little community with their labourers, frequently exhibit this attitude very distinctly. During the slump, when the plight of the companies was evident and dismissals frequent, the production programmes were sometimes varied so as to minimise cash loss or to secure a cash profit, but prior to 1930 such changes would have been objected to by the managers.

9. There was, however, yet another and even more important consideration. This will be understood when the organisation of the estate side of the industry is remembered.

The day-to-day administration, and in practice also the larger issues of policy, of most British and Dutch rubber companies, have always been in the hands of the so-called secretarial and agency firms, British and Dutch companies and firms with branches and connections in the East. Directors of, or partners in, these agency and secretarial firms are on practically every rubber company board, of which they are the most active members. The secretarial firms rarely hold large blocks of shares of the rubber companies (differing in this respect from the secretarial firms in the mining, especially gold mining industry), and the system is one of virtually complete separation of ownership and control.

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[1] Unless bark reserves have fallen to very low levels, such a course is only justified in extreme cases, as the accumulation of bark reserves would not prevent costs from rising sharply should very severe tapping be decided on in the future.

[2] This, however, had never been true for Java, where the estates always had an unlimited supply of labour on their doorstep. Even in the other territories there were usually possibilities of moving workers from tapping to field work or vice versa.

[3] Such changes had to be carried out under the regulation schemes to meet fluctuations in exportable allowances; under the circumstances, however, they were regarded as unavoidable.

The economic interests of the secretarial firms *vis-à-vis* the operating companies, or rather their shareholders, are somewhat complex. The secretarial firms receive secretarial and agency fees, commission on sales of rubber, a number of other payments, and have contracts for estate supplies. Except in extreme cases, such as the slump of 1930–33, the rates of these payments do not vary with profits. The economic interest of the secretarial firms is generally rather to lengthen the life of the operating companies than to maximise the discounted value of net profits at any given time. During a period of very high prices which are not expected to last, the commission revenue may be sufficiently important to shift the interest of these firms towards the maximisation of the profits of the operating companies, but this would be exceptional.

The directors of the operating companies (with the exception of those members of the boards interested in secretarial firms) are generally remunerated in much the same way as most directors of public companies who have no large shareholding interest. It is reasonable to suggest on *a priori* grounds (supported by some evidence from chairmen's speeches, see footnote on page 67), that in accordance with the general trend of recent years the boards which, it will be remembered, include at least one of the directors of, or partners in, the secretarial firms, are more likely to concern themselves with the length of profitable life of the companies, than with the maximisation of their net profits in any given year, or with the maximisation of the discounted value of net profits at any given time.[1]

It should be added that Boards of Directors and the secretarial and agency firms are also concerned with the survival of the estate for social considerations, and usually feel a pride in the plantations which will make them anxious to prolong their life as far as possible.

The bias towards caution and conservatism is accentuated by the uncertainty which still surrounds the factors affecting the rate of bark renewal. Declining yields on the older estates, subsequently established as being due to soil erosion, had often been attributed to the severe tapping systems prevalent in the early days of the industry.

In short, the directors and agency firms are usually not greatly interested in adjusting output to a nicety so as to maximise their companies' profits. Moreover, because of their interest in the long life of the operating units, they are much more inclined to overestimate user cost rather than to undervalue it.

10. The answer to the question why production was maintained at a rate at which direct cash costs were much below price, is thus as follows. A user cost element involved in bark removal must be added before total marginal cost is arrived at. This element is difficult to gauge with any degree of precision, as an accurate estimate would have to assume prices, costs, rates of bark renewal, and, whenever direct taxation is heavy, the rate of taxation several years ahead, and even when some kind of estimate is made, there would be real difficulty in frequent changes in the rate of production to maximise profit by equating marginal cost (direct cash cost plus marginal user cost), with marginal revenue. At the same time key interests in the industry seem more concerned with the length of life of the companies than the maximisation of profit, and thus are content, extreme cases apart, to continue production at a rate which equates bark consumption with bark removal conservatively interpreted. Production in excess of this rate would moreover frequently result in sharply rising marginal cost.

11. Indeed, were it not for the experience of the slump, it would have been quite legitimate to omit " extreme cases apart ". The experience of Java estates during 1925 and 1926 brought out very clearly the unwillingness of administrators and managers to adjust output in accordance with changes in the market situation. At that time the Dutch estates (i.e., the companies not restricting voluntarily under the Stevenson Scheme) were confronted with a tremendous price boom of obviously temporary duration. They knew that a shilling to 1/3 per lb. was the best price they could hope for in the long run, and for a few months the price was around 4/-, with a yearly average of about 3/–. An unlimited supply of labour was at their doorstep, and nothing would have been more rational than to tap very severely for that period of exceptional prices. Yet they failed to do so, and the changes in the yield per mature acre from 1924 to 1927 on Dutch estates in Java barely showed the increase to be expected from the long-period trend of technical progress, and from the ageing

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[1] The whole concept of maximising profits becomes ambiguous whenever it is not made clear (and usually it is not), over what period profits are to be maximised. This is relevant, as present receipts and expenditure generally affect future prices, costs and profits.

of plantations which had already been classed as mature, but had not attained their highest level of productivity. No advantage was apparently taken of the exceptional opportunity.[1]

III

12. The concept of user cost was developed by Lord Keynes in the *General Theory* as part of the theory of output as a whole. It is, however, obviously relevant to the theory of value[2] and to the protests (among which Mr. Shove's have been particularly insistent[3]), against the over-simplified two-dimensional analysis in which costs vary solely with the scale of output and the equilibrium of the firm is established at the point at which marginal cost equals marginal revenue. The inadequacy of this approach emerges clearly when proper weight is given to user cost.

The user cost of a unit of output in the short period is the reduction in the discounted value of expected future quasi-rents of a piece of equipment through using it for the production of that unit of output rather than leaving it unused. The concept is one of the links between the present and the future so familiar to readers of the *General Theory*. It obviously applies to the analysis of the output of a single commodity since marginal user cost is an element in marginal cost. User cost differs however in important aspects from the other elements in marginal cost.

13. First, with user cost included, marginal cost cannot possibly be assumed to be independent of price. It is sometimes doubtful how entrepreneurs and managers estimate future quasi-rents, but it is certain that present revenues are always an important, and usually the dominant, factor. Once this is admitted, cost and supply curves containing an element of user cost cannot be regarded as independent of price or of demand, even under conditions of perfect competition (which still prevail in certain wholesale markets, especially for agricultural produce), and of homogeneity of units of factors of production.

Secondly, user cost cannot be read off a cost accounts sheet. It is necessarily a quantitatively vague and imprecise, though not for that reason unimportant, element in cost, and is definitely present in the entrepreneurial mind. The difficulty of measuring user cost is more likely to lead to its over-valuation rather than its underestimation.

Again, user cost being a link between the present and the future, inevitably introduces a time element into the cost curve. Once user cost is admitted, the time factor finds its way into the analysis, with the inevitable corollary of the period over which profits are to be maximised. Indeed, an analysis excluding time but admitting a link between the present and the future is very near a self-contradition. The omission of a time element from the theory of value becomes particularly serious when user cost is an important element in marginal cost, while at the same time decisions are taken by persons more interested in prolonging the stream of quasi-rents rather than in maximising its present discounted value. When user cost is heavy, the discrepancy of interests between managers or directors and ordinary shareholders becomes particularly marked.[4] In any event, however, references to maximisation of profits without specifying a period of time becomes meaningless once user cost is admitted as an element in marginal cost.

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[1] It would appear that the conservatism was often driven to excess and became self-defeating. The late Dr. Whitford (a leading American authority on plantation rubber) suggested in one of his Reports that the over-conservatism of the managing interests had led estates to adopt too elaborate methods of cultivation and too conservative tapping systems; in his opinion the European producers had failed to appreciate the amount of rough usage the trees would stand and by inflating their own costs had given an advantage to native producers. This appears to be borne out by an investigation into bark reserves and methods of production on Malayan native holdings, carried out in 1931/33 under the auspices of the Department of Agriculture, S.S. and F.M.S.

Company chairmen have often stated that their production policies were directed chiefly towards the maintenance of bark reserves by a rate of bark consumption not exceeding the rate of renewal. Interviews with business men in the industry and examination of documents such as visiting agents' reports confirm that this "lame and impotent conclusion" was generally acted upon.

[2] Which ". . . at least among English economists is merely a traditional misnomer for the analysis of the output of a single commodity, considered separately". Mrs. Robinson, *Economics of Imperfect Competition*, p. 16.

[3] Mr. Shove has for years past criticised this theory, chiefly by emphasising the complexity of the considerations underlying entrepreneurial decisions and the diversity of factors affecting cost of production. His views, which have so greatly influenced many who attended his lectures, do not easily lead themselves to summary; a few glimpses only can be caught from his publications, *e.g.*, *Economic Journal*, March, 1930, and March, 1933; *cf.* also his review of Mrs. Robinson's *Economics of Imperfect Competition* in *Economic Journal*, December, 1933.

[4] This clash of interests does not arise in the case of the entrepreneur proper, although even the owner-manager may, out of social considerations or for sentimental reasons, be interested in the length of life of the producing unit as distinct from a pure maximisation of the present value of all quasi-rents.

Lastly, user cost differs from cash cost in that it cannot easily be claimed as an expense for taxation purposes. The revenue authorities admit wear and tear as an element of cost, but user cost cannot be claimed so easily as can a cash expenditure. The difference may, on occasion, exercise an important influence on production policy; this can be illustrated by an example from the rubber industry.

A given quantity of rubber may be produced by various tapping methods which vary not only in cash costs but also have different effects on the rate of bark renewal, and thus involve different user costs. For instance, the rate of bark removal under the system known as fourth daily full spiral is the same as that under alternate daily half circumference tapping, and the cash tapping costs of the former are much lower since a given amount of bark can be removed in much shorter time and consequently with a much smaller labour force; the yield per acre is also somewhat higher under the fourth daily system. There is, however, still some uncertainty about the effects of the full spiral system on the growth of the tree and on the rate of bark renewal. Thus user cost under this system may be higher, and is certainly not less, than under the alternate daily method. Under heavy excess profits taxation there is thus a strong incentive to adopt the half circumference system of tapping which is less economical of labour and involves higher cash costs. The fourth daily full spiral system was developed in the late 1930's only; its merits were, however, fully appreciated by 1940. It was very little practised in 1940 and 1941 on the properties of sterling companies, practically all of which had become liable to 100 per cent. E.P.T. and thus would not have benefited from the saving in cash costs, while the directors were apprehensive of the long-run effect of the system on the growth of the trees. The failure to adopt the fourth daily system probably cost the Allies substantial quantities of badly needed rubber, as labour had become one of the bottlenecks of rubber production by 1940–41.

14. It is probable that just as in the plasticity of the cost of production, so in the importance of user cost the rubber industry presents an extreme instance[1] owing to the complete separation of ownership and control on company-owned estates, and owing to the bark removal involved in tapping. The effect of price changes on costs is particularly marked, and so is the inadequacy of the two-dimensional analysis of cost and supply curves, even though conditions of perfect competition prevail. The factors reviewed are, however, likely to be prevalent, though in varying degrees, over a wide range of industry.

APPENDIX I

In Table IV the trend of Malayan estate rubber production during the slump was illustrated by figures of total production and of output per mature acre. The latter frequently indicates the rate of capacity working much better than does total production. Capacity at any given time depends largely on past planting (five or more years previously) and the volume of production will be influenced by changes in the mature area. Total output may thus furnish a misleading idea of producers' reaction to price changes. The trend of N.E.I. native rubber exports during the slump will serve as an example.

The potential capacity[2] of N.E.I. native rubber increased by some 150 to 200 per cent. between 1929 and 1933, reflecting the heavy planting stimulated by the Stevenson boom of 1925–26. There was a particularly sharp rise from 1930 to 1931. The Singapore price of Medium Blankets (the grade into which most of the Dutch native rubber was processed) averaged 17⅛th Straits cents per lb. in 1930 and 9⅛th cents in 1931.[3] Net exports (taken as production, as no direct figures of output were available, and stocks of N.E.I. native rubber held in the interior were small) of N.E.I. native rubber totalled 89,000 long tons (dry weight) in 1930, and 88,000 tons in 1931. Looking at output only, it would appear that the supply of Dutch native rubber was almost perfectly inelastic. In terms of capacity working, however, the elasticity was much greater. According to an official Dutch estimate the 1930 production was at the rate of 60 per cent. of potential capacity, while in 1931 it had fallen to 44 per cent.[1]

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[1] With the possible exception of mining.

[2] The definition of potential capacity raises various difficulties, especially when labour rather than land is the scarce factor. It is usually defined (often implicitly) as the annual output from a given area when fully tapped at a rate at which bark consumption equals bark renewal.

[3] The fall in terms of N.E.I. currency was greater (slightly over 50 per cent.), reflecting the depreciation of the Straits dollar after September, 1931.

During the depression the various territories and classes of producer fell short of potential production in widely varying degrees, and in terms of capacity working the supply of some groups of producers was noticeably elastic, though that of the most important producers was inelastic, however expressed.

It would appear that at the time of rapid changes in productive capacity the standard definition of the elasticity of supply (small proportionate change in output over a small proportionate change in price) is likely to present, or rather to imply, a misleading picture of important aspects of the supply side. Changes in total capacity would usually be regarded as infringing the *cet. par.* postulate. It seems nevertheless that for certain purposes the elasticity of supply could be usefully expressed as a small proportionate change in the rate of capacity working over a small proportionate change in price, even though this would involve some other difficulties and complexities, both factual and conceptual.

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[1] The official Dutch *Fourteenth Report on Native Rubber Cultivation* (March, 1933) prepared by the Division of Agricultural Economics in the N.E.I. Department of Agriculture, Industry and Commerce, gave the following figures of output (net exports) and capacity working for Dutch native rubber for 1927/32.

	Production	Production as % of estimated capacity
	(*Metric tons, Dry Weight*)	
1927	100,491	100
1928	91,353	83
1929	108,584	90
1930	90,496	60
1931	88,717	44
1932	62,381	25

APPENDIX II

(A)

SUB-DIVISION OF COSTS OF PRODUCTION OF RUBBER COMPANIES FURNISHING RETURNS TO THE R.G.A. PENCE/LB.

Cos. whose Financial Year ended in	Upkeep of Mature Area	Collection, Manufacture and Despatch	General Charges	Upkeep of Buildings and Machinery	Depreciation of Buildings and Machinery	Head Office Expenses	TOTAL
1929							
1st Quarter ..	0·70	2·64	2·28	0·09	0·40	0·47	6·58
2nd ,, ..	0·70	2·58	2·18	0·11	0·29	0·45	6·31
3rd ,, ..	0·63	2·50	2·06	0·07	0·38	0·24	5·88
4th ,, ..	0·84	2·51	1·87	0·07	0·35	0·29	5·93
1930							
1st Quarter ..	0·83	2·55	2·07	0·09	0·36	0·42	6·32
2nd ,, ..	0·94	2·57	2·22	0·10	0·26	0·42	6·51
3rd ,, ..	0·71	2·33	2·04	0·04	0·38	0·20	5·70
4th ,, ..	0·73	2·13	1·81	0·05	0·36	0·27	5·35
1931							
1st Quarter ..	0·58	2·01	1·76	0·05	0·29	0·35	5·04
2nd ,, ..	0·39	1·82	1·60	0·03	0·23	0·34	4·41
3rd ,, ..	0·36	1·57	1·43	0·02	0·37	0·18	3·93
4th ,, ..	0·26	1·47	1·27	0·02	0·25	0·21	3·48
1932							
1st Quarter ..	0·26	1·29	1·14	0·02	0·11	0·27	3·09
2nd ,, ..	0·20	1·20	1·19	0·02	0·14	0·27	3·02
3rd ,, ..	0·22	1·24	1·15	0·02	0·33	0·17	3·13
4th ,, ..	0·14	1·17	1·03	0·02	0·23	0·19	2·78

The first four columns add up to the f.o.b. cost and their total is the same as Col. (*b*) of Table I of the text. The very sharp fall in the cost of estate upkeep and the much smaller proportionate fall in Head Office expenses are in accordance with *a priori* expectations.

APPENDIX II

(B)

STERLING COMPANY COSTS BY COUNTRIES. PENCE/LB.

Cos. whose Financial Year ended in	Malaya	Java	Sumatra	Ceylon	India and Burma	Borneo	ALL COUNTRIES
1929							
1st Quarter ..	6·23	6·10	7·60	8·56	7·08	7·77	6·58
2nd „ ..	5·97	6·65	6·97	7·21	7·73	6·88	6·31
3rd „ ..	5·72	5·42	6·29	—	—	9·05	5·88
4th „ ..	5·94	5·53	5·83	6·20	7·21	8·73	5·93
1930							
1st Quarter ..	6·03	5·53	7·29	7·55	6·94	7·52	6·32
2nd „ ..	6·13	6·78	7·44	7·34	7·58	7·20	6·51
3rd „ ..	5·26	5·30	6·95	—	—	8·24	5·70
4th „ ..	5·34	5·09	5·31	5·61	5·78	7·62	5·35
1931							
1st Quarter ..	4·75	4·36	6·22	5·73	5·95	5·65	5·04
2nd „ ..	4·13	4·59	5·16	4·41	6·29	4·97	4·41
3rd „ ..	3·64	3·53	4·89	—	—	4·69	3·93
4th „ ..	3·46	3·21	3·58	3·81	3·60	5·08	3·48
1932							
1st Quarter ..	2·90	2·99	3·60	4·02	4·72	3·54	3·09
2nd „ ..	2·73	3·61	3·85	3·21	4·14	2·64	3·02
3rd „ ..	2·79	3·79	4·15	—	—	5·37	3·13
4th „ ..	2·47	3·60	3·26	2·76	3·40	2·06	2·78

The Sumatra and the Java figures refer only to British Companies operating there and these generally had higher costs than their Dutch or American rivals.

APPENDIX II

(C)

OUTPUT OF RUBBER OF STERLING COMPANIES FURNISHING COST RETURNS TO THE R.G.A.

Financial Year Ending in	Output 000 lb.	Yield per Mature Acre lb.
First Quarter		
1929	48,748	331
1930	55,086	365
1931	52,771	346
1932	70,682	373
Second Quarter		
1929	54,361	363
1930	55,006	361
1931	58,267	377
1932	53,713	382
Third Quarter		
1929	50,813	389
1930	53,784	399
1931	53,882	401
1932	54,552	377
Fourth Quarter		
1929	226,362	418
1930	224,923	406
1931	230,512	406
1932	222,828	411

The companies included in the return operated in a number of territories in several of which the age of maturity of rubber trees differ. In Malaya, the tappable age of seedling trees is usually regarded as five years from planting, while in some other territories the figure is higher. There was heavy new planting in the mid 1920's and the areas planted are included in the Malayan mature acreage of the early 1930's, while they were excluded in some of the other territories. This largely explains the apparent discrepancy between the trend of output per mature acre shown here and that in Table IV of the text.

Output, Labour and Machines in the Coal Mining Industry in Great Britain

By E. C. Rhodes

THE quarterly statistical summaries of the Mines Department, issued before the war, giving data for each coal-mining region in Great Britain enable us to observe the changes with time in the output per manshift worked at the coal face. The annual reports of the Mines Department contain information about the number of coal-cutting machines in use in the mines and about the average weekly number of coal-winding days, month by month. From these figures, some indication of the utilisation of machines at the coal-face is possible. One method of indicating the increasing importance of machinery is to quote the changing proportions of coal cut by machinery to the total output.

PERCENTAGE OF TOTAL OUTPUT CUT BY MACHINES.

Year	Northumberland	Durham	Yorkshire	N. Derby, &c.	S. Derby, &c.	Lancashire, &c.	S. Wales, &c.	Scotland
1922 ..	21	12	13	14	12	15	4	40
1923 ..	25	14	14	15	13	17	4	45
1924 ..	28	16	16	17	15	17	5	47
1925 ..	29	16	16	19	16	18	6	50
1927 ..	42	18	17	22	22	20	7	56
1928 ..	51	21	18	25	27	25	8	59
1929 ..	56	22	18	28	30	29	9	63
1930 ..	64	26	20	34	34	33	10	66
1931 ..	70	29	24	41	45	38	12	66
1932 ..	75	33	26	41	49	45	12	69
1933 ..	81	35	30	51	52	51	13	72
1934 ..	83	37	38	60	55	57	15	75
1935 ..	84	38	44	67	61	63	18	77
1936 ..	88	39	47	73	65	68	21	79
1937 ..	91	40	51	77	68	71	24	79
1938 ..	91	42	56	78	70	76	26	80

In this table, the main regions are shown separately. N. Derby, &c., stands for North Derby and Nottinghamshire; S. Derby, &c., stands for South Derby, Leicestershire, Cannock Chase and Warwickshire; Lancashire, &c., for Lancashire, Cheshire and North Staffordshire; S. Wales, &c., for South Wales and Monmouth.

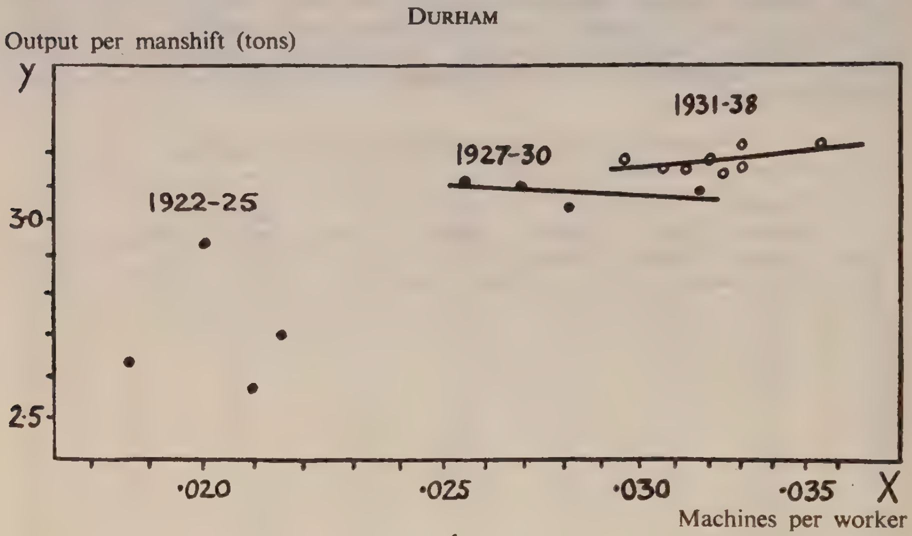
The year 1926 is left out on account of the long stoppage in coal mining in that year.

We would like to emphasise the fact that there has been, in the period 1922–38, a considerable increase in the percentage of coal cut by machines in all districts. Secondly, the percentages vary from region to region. In 1938, S. Wales, &c., with 26 per cent. contrasts with Northumberland with 91 per cent.

In this same period, 1922–38, there has been an increase in the output per manshift worked at the coal face in all districts. It is interesting to try to link up the increased employment of machinery in getting coal with the increase in the output for a unit of labour. There are certain matters which have to be considered before we can proceed further.

The output has been related to the work done at the coal face rather than to all the work done in the pit. In the quarterly summaries, the Mines Department gives figures relating to manshifts worked "elsewhere below ground" other than at the coal face, and figures for the manshifts worked "on the surface". In the short run, of course, the proportions of labour at the coal face, elsewhere below ground and on the surface are fairly constant for a particular district. From our point of view, however, as we wished to use the data for coal-cutting machines, it seemed sensible to restrict ourselves to the data respecting labour at the coal-face. We wish to concentrate on figures which relate to certain labour and the machines used by that labour.

The figures in the quarterly summaries relate to practically the whole output of each district, but not to all. This fact is always mentioned in each quarterly summary. As the percentages are in the neighbourhood of 95–99, not much harm is done when we link the data from the quarterly summaries with those from the annual reports relating to machines. These annual reports include data for the whole industry. Again, the figures for coal-cutting machines in use are representative figures for each year; we have no week by week detail of changes. As the labour figures we intend to use are those for the number of manshifts worked, *i.e.*, data concerning the use of labour, we need corresponding figures for the use of machines. The most reasonable method seems to be to multiply the number of machines in use (the annual figure) with the total number of coal-winding days in the year. What we need is the total number of machine-shifts corresponding to the number of manshifts, but such a figure is not available. The next best thing appears to be the figure proposed. For labour use, we therefore have the number of manshifts worked at the coal face; for machine use, we have the number of coal-cutting machines multiplied by the number of coal-winding days.



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We have to remember a little of the recent history of the coal industry. The length of the shift has changed in the years between the wars on various occasions. One of the reasons for an increase in the output per manshift is an increase in the shift from 7 hours to 7½ hours or 8 hours. We find ourselves forced to use the available data in sets of years; 1922–25, 1927–30, 1931–38 in general, but for two districts we have longer runs, 1927–38.

We have confined our attention to eight districts, which between them account for the bulk of the output of coal in Great Britain.

The figures in the following Table I are annual figures for output per manshift at the coal face and figures for machines per worker at the coal face, obtained by dividing the number of coal-cutting machines multiplied by the number of coal-winding days by the number of manshifts worked at the coal face.

TABLE I

OUTPUT, LABOUR AND MACHINES

(*a*) Output per manshift (tons)
(*b*) Number of machines per worker

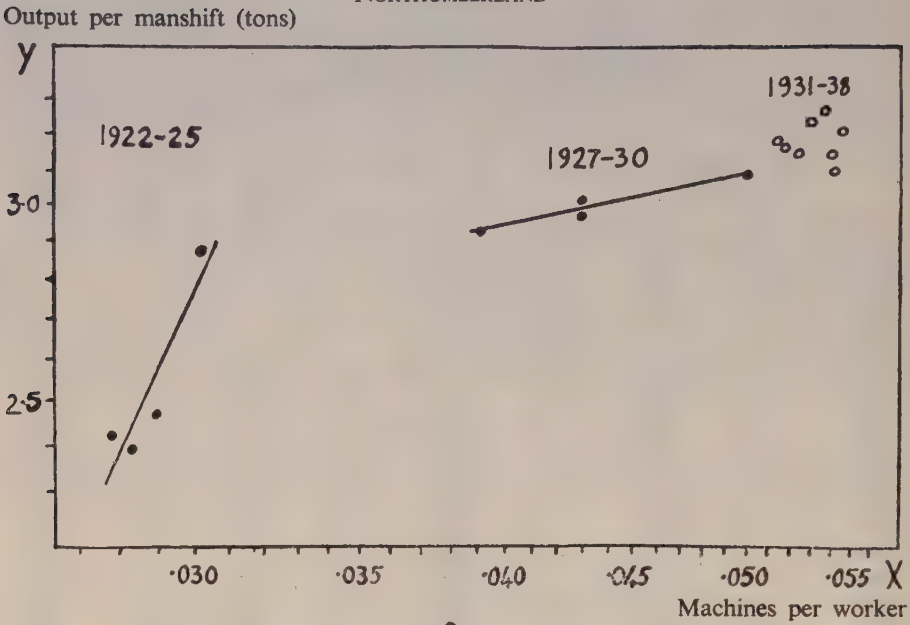
	North-umberland		Durham		Yorkshire		N. Derby, &c.	
	(*a*)	(*b*)	(*a*)	(*b*)	(*a*)	(*b*)	(*a*)	(*b*)
1922 ..	2·87	·0302	2·93	·0201				
1923 ..	2·42	·0278	2·63	·0187				
1924 ..	2·39	·0283	2·57	·0209				
1925 ..	2·47	·0289	2·70	·0215				
	—	—	—	—				
1927 ..	2·92	·0391	3·11	·0255	2·79	·0138	2·78	·0147
1928 ..	2·97	·0430	3·10	·0268	2·82	·0133	2·84	·0156
1929 ..	3·02	·0430	3·03	·0281	2·91	·0136	2·91	·0169
1930 ..	3·08	·0500	3·08	·0317	2·97	·0141	2·97	·0175
1931 ..	3·15	·0524	3·22	·0329	3·02	·0153	3·03	·0177
1932 ..	3·18	·0515	3·23	·0355	3·10	·0144	3·12	·0174
1933 ..	3·21	·0545	3·16	·0330	3·24	·0163	3·31	·0193
1934 ..	3·27	·0538	3·13	·0324	3·38	·0177	3·45	·0204
1935 ..	3·24	·0530	3·18	·0320	3·54	·0197	3·56	·0215
1936 ..	3·16	·0518	3·15	·0313	3·56	·0190	3·63	·0227
1937 ..	3·14	·0540	3·15	·0306	3·54	·0207	3·72	·0244
1938 ..	3·10	·0542	3·18	·0291	3·55	·0208	3·76	·0242

	S. Derby, &c.		Lancashire, &c.		S. Wales, &c.		Scotland	
	(*a*)	(*b*)	(*a*)	(*b*)	(*a*)	(*b*)	(*a*)	(*b*)
1922 ..			2·02	·0197	1·92	·00350	2·30	·0251
1923 ..			2·02	·0225	1·90	·00393	2·18	·0247
1924 ..			1·96	·0234	1·87	·00451	2·15	·0268
1925 ..			1·98	·0221	1·87	·00476	2·27	·0293
			—	—	—	—	—	—
1927 ..	2·76	·0168	2·20	·0241	2·20	·00565	2·71	·0353
1928 ..	2·84	·0156	2·29	·0267	2·22	·00591	2·85	·0401
1929 ..	2·96	·0180	2·38	·0265	2·19	·00600	2·85	·0400
1930 ..	3·03	·0187	2·43	·0283	2·16	·00689	2·83	·0406
1931 ..					2·11	·00724		
1932 ..	2·99	·0216	2·58	·0309	2·14	·00736	2·89	·0389
1933 ..	3·10	·0224	2·69	·0332	2·17	·00805	2·91	·0391
1934 ..	3·24	·0237	2·81	·0360	2·22	·00865	2·92	·0398
1935 ..	3·34	·0255	2·95	·0379	2·31	·00918	2·94	·0418
1936 ..	3·37	·0272	3·00	·0406	2·45	·01043	2·85	·0408
1937 ..	3·40	·0277	2·90	·0389	2·38	·00967	2·78	·0404
1938 ..	3·45	·0290	2·89	·0367	2·35	·01000	2·74	·0402

The gaps in this table occur owing to the lack of available comparative data.

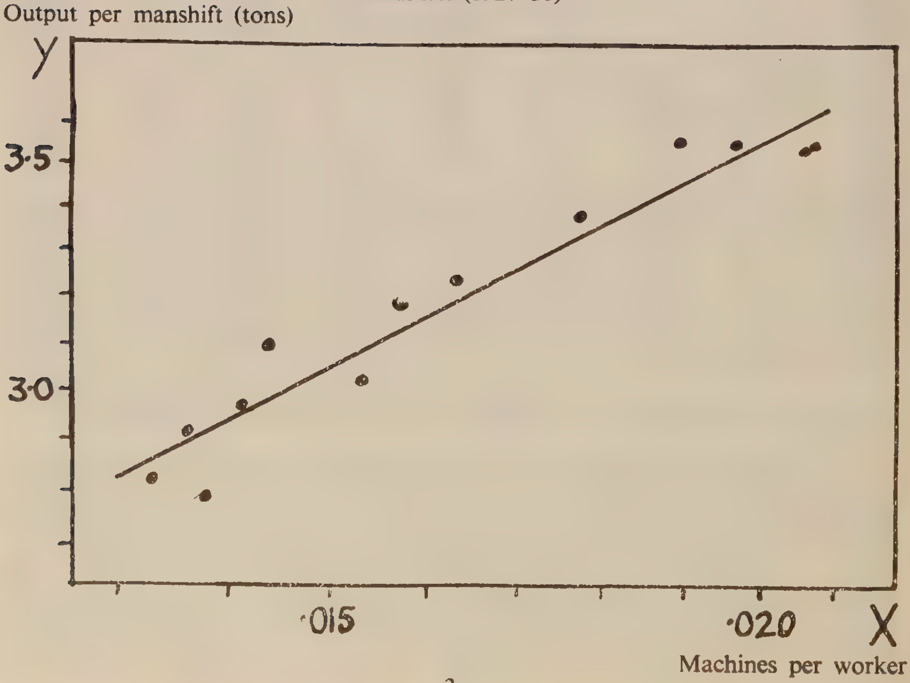
As we stated above, there has been an increase in the average number of tons produced per manshift at the coal-face. Some of this increase is due to increasing the length of the shift, but some is due to an increasing use of coal-cutting machinery. This can be seen in the data for Yorkshire and N. Derby, &c., quite clearly. Here we have a fairly long run of years, 1927–38. For Yorkshire the output per manshift increases from 2·79 tons in 1927 to 3·55 tons in 1938, the corresponding figures for machines per worker are ·0138 and ·0208 For N. Derby, &c., the output figure increases from 2·78 tons to 3·76 tons corresponding to an increase in the machines per worker from ·0147 to ·0242. For S. Derby, &c., there are increases in both factors in the two periods 1927–30 and 1932–38. For Lancashire, &c., in the period

NORTHUMBERLAND



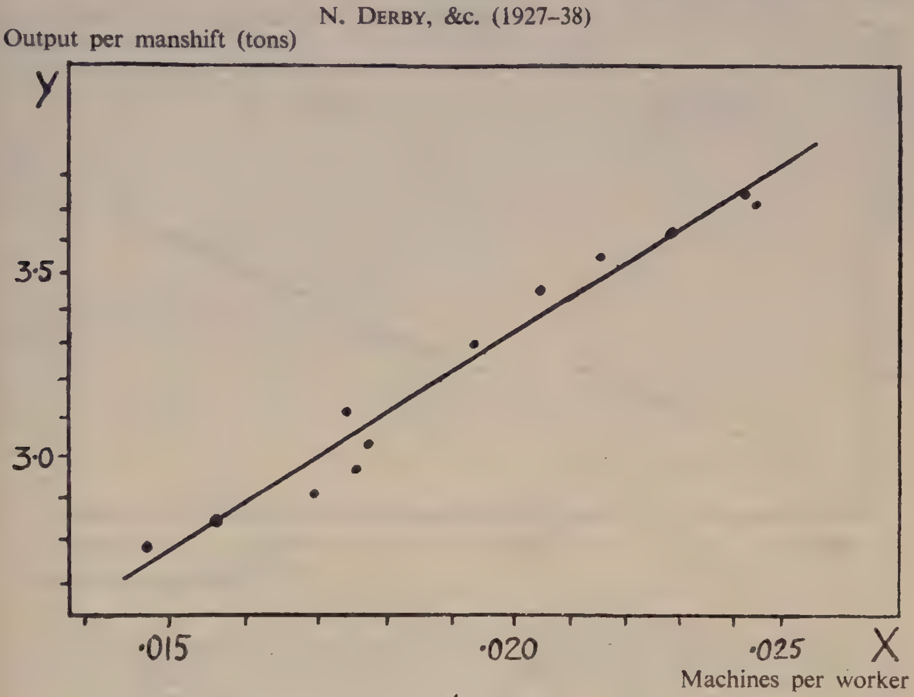
2

YORKSHIRE (1927–38)



3

1922–25 there is a slight decline in the output figure and an increase in the number of machines per worker, but for 1927–30 and 1932–38, both factors increase. For S. Wales, &c., there is a slight decline in the output figure for both 1922–25 and 1927–30, with increases in the number of machines per worker, but for 1931–38 both factors increase. For Scotland, from 1922 to 1925, a slight decline in the output figure goes with an increase in the machine figure; from 1927 to 1930 there are increases in both; and from 1932 to 1938 a decline in output accompanies an increase in the machine figure. For Northumberland, from 1922 to 1925 there is a fall in output with a fall in machines; from 1927 to 1930, both rise together; from 1931 to 1938, the output rises, then drops, the machine figure rises, drops and rises again. For Durham, from 1922–25, there is a fall in output accompanied by a rise in machines per worker; a slight drop in output from 1927–30 goes with a rise in the number of machines; from 1931 to 1938 there is a drop in output with a drop in the number of machines per worker.



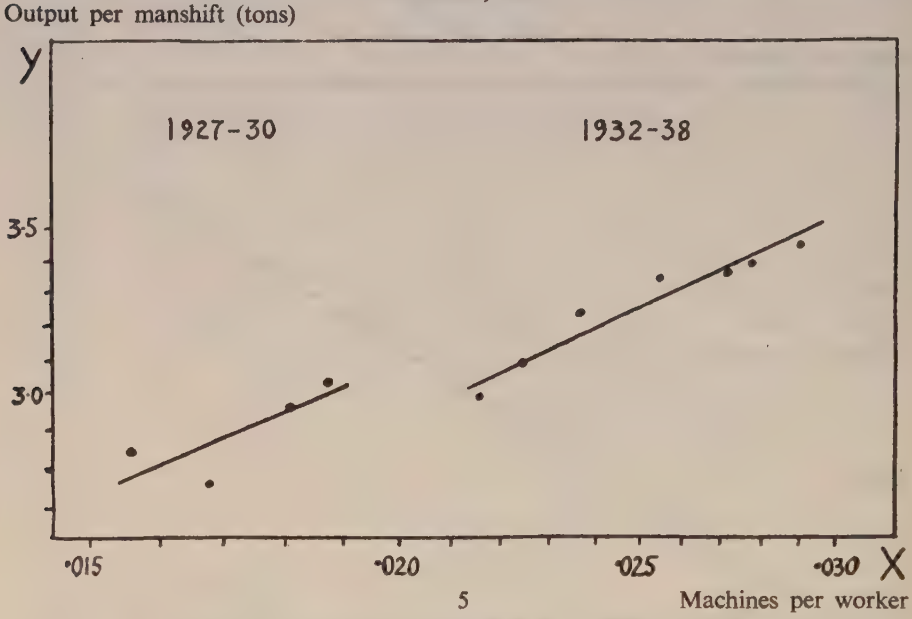
4

When we study these figures, some of which behave in an apparently erratic manner, we have to remember another factor which so far has not been mentioned, the time element. During the period under review the coal-mining industry has passed through difficult times. 1923 was a boom period owing to difficulties on the Continent, in 1926 occurred the great stoppage, 1929 was comparatively a boom year, 1931–33 saw the world-wide depression with subsequent recovery to 1937, a boom year. Some mines ceased to operate, new mines began work. Over a long period, we would anticipate increasing difficulties of getting coal, and if the methods of operation remained the same we would expect a decline in the output per manshift. It is impossible for us, with our data, to assess the change in output per manshift with time, other things being equal. We can only suppose that in a short period of a few years this factor may be unimportant, but we may be wrong.

What we must do is to assume that the time factor is negligible, that any changes we observe are due only to changes in the use of machinery, remembering all the time that this initial assumption may be unjustified. In order to see, then, as clearly as possible, the changes in output per manshift with the changes in the number of machines per worker, without paying any attention to the time element, the figures in Table 1 have been graphed and are shown in the diagrams 1 to 8. These diagrams show the output per manshift (y) and the number of machines per worker (x) on logarithmic scales.

Where the points plotted in the diagrams appear to give a fair indication of a connection between the values of x and y, this is shown by a line drawn through the set of points. In a few cases, the points are clustered in such a manner that no connection appears to exist between x and y. This happens for Durham (1922–25), Northumberland (1931–38), and Scotland (1932–38). y increases with x for Northumberland (1922–25) and (1927–30), for Durham (1931–38), for Yorkshire, for N. Derby, &c., for S. Derby, &c., for Lancashire, &c. (1927–30) and (1932–38), for S. Wales, &c. (1931–38), for Scotland (1922–25) and (1927–30). There are some cases when y diminishes as x increases; Durham (1927–30), Lancashire, &c. (1922–25), S. Wales, &c. (1922–25) and (1927–30). The decline in y with increasing x in these cases seems to be very slight. Also some of the increases in y with increasing x are small. The slopes of the lines drawn indicate the rate of change of y with x. It would perhaps be fairer to state that in these cases y is for practical purposes the same for different values of x. Certainly, with such small numbers of points in a diagram of this kind, the slope of the line indicating connection between y and x would have to be very pronounced before credence could be given to it. The computed gradients of the lines are shown in Table II.

S. Derby, &c.



5

TABLE II

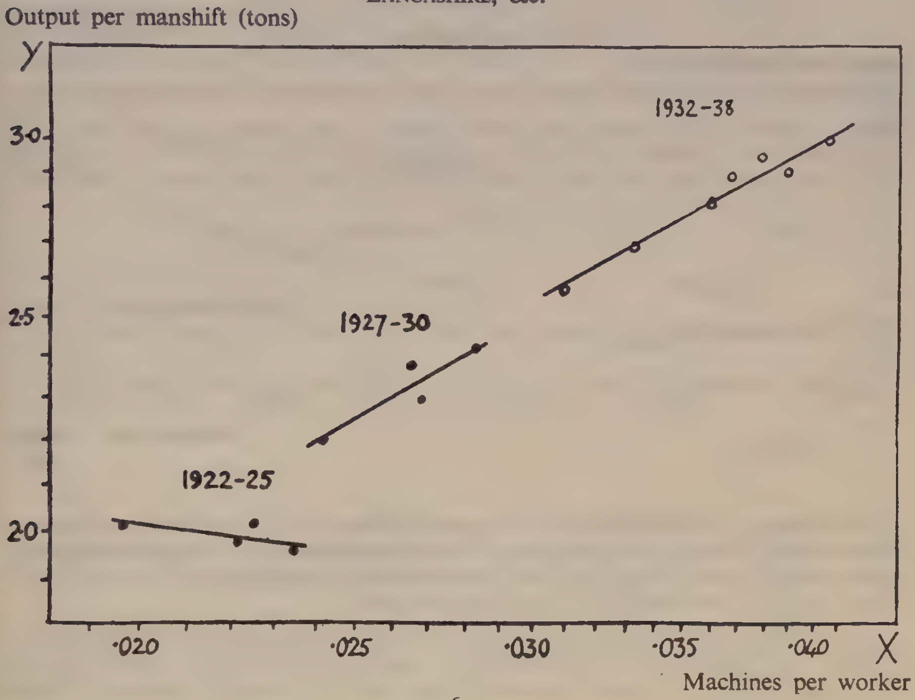
GRADIENTS OF LINES CONNECTING y WITH x

	1922–25	1927–30	1931–38	1932–38	1927–38
Northumberland ..	2·204	·210			
Durham		– ·046	·087		
Yorkshire					·537
N. Derby, &c.					·644
S. Derby, &c.		·398		·453	
Lancashire, &c.	– ·125	·599		·563	
S. Wales, &c.	– ·091	– ·102	·379		
Scotland	·059	·360			

Of the gradients shown in this table, those for Yorkshire, N. Derby, &c., S. Derby, &c., Lancashire, &c. (1927–30) and (1932–38), S. Wales, &c. (1931–38), Scotland (1927–30) probably have some significance. The large value for Northumberland (1922–25) is peculiar. This will be referred to later. The negative values for Durham (1927–30), Lancashire, &c. (1922–25), S. Wales, &c. (1922–25), and (1927–30) are probably not significant; and the small positive values for Northumberland (1927–30), Durham (1931–38), Scotland (1922–25) are probably without real significance. The average of the larger positive values to which we attach significance is ·49, leaving out the high Northumberland figure.

Apart from Northumberland, the period 1922–25 does not give any really significant results. The figures for Lancashire, &c., and S. Wales, &c., show on the average a slight fall in output per manshift with increasing use of machinery, but this is probably not really significant. For Durham, no real connection between output per manshift and the use of machinery can be discerned for any of the periods.

LANCASHIRE, &c.



6

If we combine together all the positive indications of a connection between the output and machinery use, we can write the result in the form

$$y = A\ x^{b},$$

where A is a constant and b appears to have the value ·49.

If we designate output by P, number of manshifts worked at the coal-face by L, number of machines in use multiplied by the number of coal mining days by M, this equation may be written in the form,

$$\frac{P}{L} = A\left(\frac{M}{L}\right)^{b},$$

and this may be transformed into

$$P = A\ L^{1-b}\ M^{b}.$$

We have thus arrived at a form of production function. In general, the production function is written as

$P = f\ (x_1,\ x_2,\ x_3,\ .\ .\ .)$, where $x_1,\ x_2,\ x_3,\ .\ .\ .$ are the factors which enter into production, P is the resultant product, and f indicates the connection between the product and the factors of production. If the factors of production are reduced to two, labour and capital, the production function is written as

$$P = f(L,\ C),$$

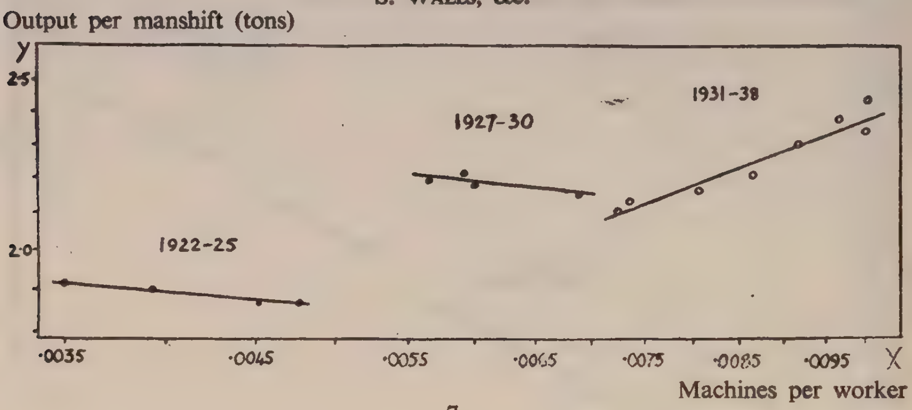
where L stands for labour and C for capital. The production function in the following definite form has been proposed,

$$P = A\,L^{a}\,C^{b}.$$

This is the form which has emerged from our work, except that we have replaced capital by machinery use, and all labour by labour at the coal-face.

The production function enters into theory as a device for considering the behaviour of an entrepreneur at an instant of time. With changing time and subsequent changing conditions the form of production function may or may not change.

S. WALES, &c.



7

In our discussion, we have ignored the possible effects of time on the conditions of the coal mining industry, and have deduced that apparently the connection between output and labour and machinery may approximately be indicated by $P = A\,L^{1-b}\,M^{b}$. The $a + b$ of the theoretical form is unity in our case.

It is possible to test this in the following manner: The coal mining industry responds to seasonal changes in demand for coal by increasing or decreasing the amount of time spent by workers in the pits. The seasonal movement in the number of coal-winding days is nearly coincident with that in the average number of shifts worked. Therefore, in a short period movement of a seasonal character, L, the number of manshifts worked and M, which may be called the number of machine-shifts worked, vary together, the ratio of L to M being constant. So for a short period of this kind the equation

$$P = A\,L^{1-b}\,M^{b}$$

may be written

$$P = B\,L,$$

where B is another constant. This is obtained by substituting for M, $C\,L$, where C is a constant. Thus if p represents a small increment in P due to a small increment in L of amount l, we shall have the equality of the proportionate increase in P to that in L, i.e.,

$$\frac{p}{P} = \frac{l}{L}$$

We can, from the quarterly summaries, relate the proportionate changes in output to the proportionate changes in manshifts worked at the coal-face, from the changes between summer and winter demand for coal. If the equation deduced from the annual figures is correct, these proportions should be equal. We find that the ratio $\frac{p}{P} \div l/L$ is not exactly unity. The results of our computations are given in Table III.

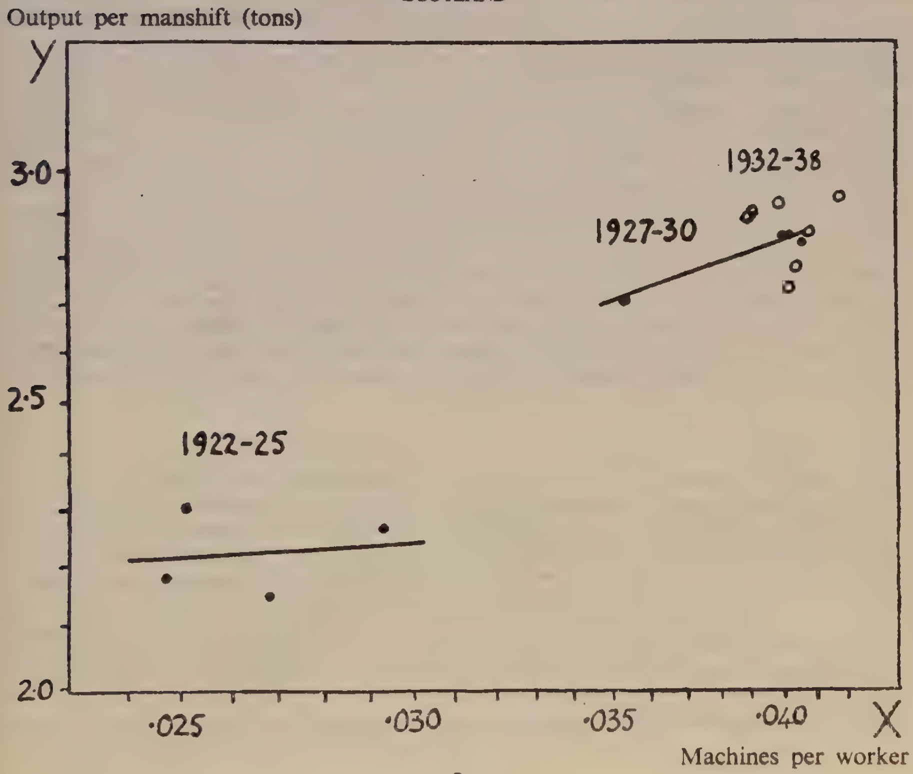
TABLE III

RATIO OF p/P TO l/L FROM SEASONAL FIGURES OF OUTPUT AND MANSHIFTS WORKED.

	1922–25	1927–30	1931–38	1932–38	1927–38
Northumberland	·919	1·044	1·017		
Durham	·953	1·021	1·025		
Yorkshire					1·102
N. Derby, &c.					1·024
S. Derby, &c.		1·081		1·092	
Lancashire, &c.	1·139	1·102		1·098	
S. Wales, &c.	1·117	1·118	1·060		
Scotland	1·040	·988		1·013	

It will be seen that the ratios in this table are for the greater part slightly larger than unity. This seems to suggest that the industry is working in conditions of increasing returns. But it would not be right to pay too much attention to the exactness of these figures, as the statistical methods used may influence them. We think that the better course would be to stress the fact that these figures are all fairly near unity, and to agree that the evidence of this table supports the form of the production function obtained from the previous data.

SCOTLAND



8

It is necessary to mention again the high figure for Northumberland (1922–25) in Table II. This figure was 2·204. If we take this roundly as 2, we should have

$$y = A\,x^2,$$

leading to $P = A\,L^{-1}\,M^2$. This seems rather ridiculous, as it would mean that for a given amount of machine use, an increase in labour use would diminish output, a veritable case of too many cooks spoiling the broth. The high figure 2·204 is due to the 1922 figures for output per manshift and for the number of machines per worker being large compared with the figures for the other three years. It would probably be wise to neglect this unusual result; it is very different from the other figures of Table II.

We have neglected the influence of time. It may be that the differences between districts revealed in our results are due to this. We find that an increasing use of machinery in Durham is unaccompanied by any real change in output per manshift. It may be that the increasing use of machinery is just sufficient to counterbalance the increasing difficulties of coal-getting as the years pass. What we need is more exact information, especially information relating to the same pits. We are able to make computations for the different districts—we ought to confine our attention to individual pits.

It appears that for the whole industry an increase of 10 per cent. in the use of machinery gives about 5 per cent. increase in output for the same labour.

I have to thank Mr. C. E. V. Leser for assistance in the earlier stages of this work. His services were made available through the kind offices of the Economic Research Division of the London School of Economics.

Thomas Chalmers on the Public Debt

By R. O. Roberts

Thomas Chalmers (1780—1847) the theologian and sociologist is a figure of considerable importance in the history of the doctrine of public finance, if only because in 1808 in his *An Enquiry into the Extent and Stability of National Resources*[1] he argued for a taxation of " the excess of income above that which is laid out in purchasing the necessaries of existence ", and, at this early date, argued that there should be a personal (tax-free) allowance in income taxation.[2] Moreover, far from being opposed to practically all State expenditure, like his predecessor Adam Smith and his contemporary Ricardo, he constantly argued in favour of a judicious public expenditure. Thus, in 1808, he wrote that: " The tax which is paid to government is always felt and considered as a diminution of income: it is looked upon as lost to the individual whereas he ought to consider that it is perhaps that part of his income which is laid out to the greatest advantage even for his own personal enjoyments ".[3] In 1832, criticising Parnell's zeal for retrenchment as expressed in the essay *On Financial Reform*, he emphasised that " if taxes were rightly laid, and the produce of them rightly expended, they admit of being most beneficially increased for the best interests of the nation; and with no other sacrifice, than a sacrifice of luxury and splendour on the part of the landed proprietors ".[4]

Possibly, however, no British writer on any economic topic has been more continuously criticised without justification than Thomas Chalmers. It would, indeed, be difficult to find, in the whole history of economic doctrine, a more striking example of misinterpretation than that which Chalmers's statements on the National Debt have endured in the works of later writers on public finance. In what follows Chalmers's theory of public borrowing will be traced through his economic writings; he will be shown to have held the view (fairly near to the one which is being increasingly adopted), that the abstraction of capital from private enterprise by public borrowing raised prices and reduced consumption, and particularly the consumption of luxuries. Erroneous statements regarding Chalmers's theory from the works of John Stuart Mill, Bastable, Edgeworth, and the American, Matsushita, will then be considered; these statements make Chalmers's theory dependent on a strict adherence to the wage-fund doctrine whereby supposedly he held that a public loan was raised out of the wage-fund, thus reducing wages and bearing exclusively on the working classes. An attempt will be made to account for this gross misinterpretation.

I

In a chapter " On the Effects of Taxation upon the Labouring Classes of the Community " published in the book of 1808, Chalmers stated categorically: " It may appear a paradox, but it is no less true, that it is not in the power of government permanently to abridge the comfort of labourers by any mode of taxation which it might think proper to devise; that they are the very description of people whom it is not only not in the interest but even not in the power of government to force to surrender a single fraction of their incomes to the necessities of the state . . .".[5] Taxes, he believed, were ultimately paid by the " landed proprietor ", and the funds for the maintenance of the workers were not at all diminished by taxation; these disposable funds were only redistributed between the landowners and the government through taxes in such a way that the government's ability to employ labour was increased.[6]

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[1] Loc. cit., p. 272.

[2] *Ibid.*

[3] *Ibid.*, p. 229.

[4] *On Political Economy in Connection with the Moral State and Moral Prospects of Society* (Glasgow, 1832), p. 302, footnote; in *Works*, Vol. XIX, p. 305, footnote.

[5] *Extent and Stability of National Resources*, p. 253.

[6] *Ibid.*, pp. 262–263. Cf. Letter of December 4th, 1807, to his brother James Chalmers in *Chalmers's Memoirs*, by his son-in-law, the Rev. William Hanna, LL.D. (Edinburgh, 1854), Vol. I, pp. 90–91.

The only discrimination against the workers that Chalmers mentioned—and deprecated—in this early treatment of public finance was the inferior pay which the labourers received in the semi-conscript British army of the time.[1]

A reviewer in *The Farmer's Magazine*[2] complained that Chalmers contemplated a taxation " far above the rental of the whole landed property in the island, though proposed to be levied on the luxuries of those who are in possession of that property " —a complaint perhaps justified by Chalmers's undue emphasis on the share of the landowners in the disposable national income. He sought, rather lamely, to reply to this charge in his *Political Economy* of 1832 by admitting the fund-holders as joint proprietors of the land, thus increasing the taxable wealth by a half, and by showing that the abolition of indirect taxation would reduce both the cost of living of the " affluent " and " the expenses of husbandry ".[3]

Chalmers elaborated his defence of public expenditure in *The Christian and Civic Economy of Large Towns* (Glasgow, 1826), a book which he later re-christened *The Christian and Economic Polity of a Nation.* He considered that Adam Smith had exaggerated the evils of national indebtedness by arguing that it involved a net loss of capital to the nation.[4] He supposed the case of a capitalist who squandered a portion of his capital: " A fund out of which a certain population received the wages of their industry, and a certain addition was made to the annual produce of labour of the country has now been dissipated ".[5] He attempted to show that there were two ways by which the deficiency could be repaired. The first rested on the assumption that the remaining employers could and would re-employ in the same trade the workers who could no longer be employed by the spendthrift employer; the same quantity of produce would be taken to market as before, demand would remain the same and the country's capital would be restored in a single year: " The capital in this case is repaired at the expense of one year's reduced wages to the labourers ".[6]

The second theory of the method of restoring the lost capital assumed that the discharged workers sought employment in a new trade. Less would be produced in the original trade and the price of the produce would rise sufficiently to restore the capital. Here again Chalmers unrealistically assumed demand conditions to remain unchanged. " In the second way of it, it (*i.e.*, capital) is repaired at the expense of the consumers. Whether in either way exclusively, or (which will happen most frequently) by a mixture of them both, it will recover, and in a single year, the magnitude from which it had fallen."[7] Similarly, he held, " the capital absorbed by the national debt is replaced by the same speedy operations with the capital that has been dissipated by extravagance ".[8] This recovery by " a *vis medicatrix* in the economic machine itself ",[9] he explained to be the result of a rise of profits consequent upon a rise in prices and/or lower wages.[10]

In the *Political Economy* (1832) Chalmers made no mention of the agency of low wages in repairing the loss of capital and concentrated on the diminution in the supply of commodities and the rise of prices: " Capital is repaired, and that almost *per saltum*, not by means of parsimony on the part of capitalists, but by means of high prices for a year, which is tantamount, we admit, to a privation for that time on the part of customers. Never are capitalists more exempted from the duties and cares of frugality than when this restorative process is going on for to them it is a season of hey-day prosperity ".[11] Chalmers adduced in proof of his theory " the price of stocks—which fall in war sometimes to half of their value in peace; and indicating, therefore, no less than a double rate of profit ".[12] After a desolating war, he observed, a greater than ordinary proportion of the people would devote themselves to the production of food and necessities; later they would engage in the production of " second necessaries ". " By one revolution of the economic cycle what is termed *the circulating capital* would be nearly restored; and by a few revolutions more, what is termed *the fixed capital* would be fully restored; and all this not by the parsimony of successive generations, but by the privations of a very few successive seasons."[1]

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[1] *Extent and Stability of National Resources*, pp. 280–281.

[2] June, 1808, p. 239.

[3] Op. cit., *Works*, Vol. XIX, pp. 291 ff.

[4] Loc. cit., p. 303.

[5] *Ibid.*, pp. 304–305.

[6] *Ibid.*, p. 307.

[7] *Ibid.*, pp. 311–312.

[8] *Ibid.*, pp. 313.

[9] *Ibid.*, p. 317.

[10] *Ibid.*, p. 315.

[11] *Works*, Vol. XIX, p. 137.

[12] *Ibid.*, p. 138. Cf. Vol. XX, p. 78.

Note D appended to the *Political Economy*[2] elaborated the argument of the text and of the *Christian and Civic Economy of Large Towns* because of " its immense practical importance, if true, as being decisive . . . of the prodigious infatuation, nay, insanity of the funding system ".[3] Here again he repeats the argument that the prices of commodities and, consequently, profits, rise as a result of the abstraction of capital by borrowing.[4] In this Note, however, a new line of thought occurs[5] where Chalmers seeks to assess the relative merits of taxation and public loans as agents of extraordinary public finance; but there is only one overt indication (the footnote on Parnell, *On Political Reform*, in *Works*, Vol. XIX, p. 305) that Chalmers was writing under the influence of the tax-loan controversy which was then raging.

The conclusion which Chalmers reached was that public borrowing was " doubly more burdensome to the country " than taxation. This he attempted to prove: " Should government borrow twenty millions for the exigencies of the current year, there are, in that year, twenty millions worth less of commodities brought to the general market than there would otherwise have been ".[6] If the government had raised the same amount by additional taxes " the community no doubt, would have had twenty millions less to spend; but they would not have had the high prices to encounter, which are consequent on those abstractions of capital produced by government loans ".[7] Taxation and borrowing raised the same sum from the public, in whose condition there was " no real difference, whether bereft of a given amount of enjoyment by means of higher prices or higher taxes ".[8] The system of borrowing, however, left a debt which had to be settled in the future: " It is thus that the national debt has all been virtually paid once, and yet remains to be paid over again—paid, in the first instance, as it was contracted, by the high prices consequent on a deficient capital; and to be paid in the second instance to the actual creditors, either by a liquidation of the principal, or by a perpetual interest ".[9] Whereas the same burden was laid on the community at the time both by taxation and by borrowing, the method of borrowing left the heritage of " a great national mortgage which remains to oppress and enfeeble the country "[10] Notwithstanding this, Chalmers observed that loans were more popular than taxes because their supposed double mischief was disguised. " The people do not feel that government are taking the money out of their pockets by borrowing, though this is virtually done, and at the same time too, in the shape of higher prices, if not in the shape of higher taxes ".[11] Taxation would have the added advantage over borrowing that it would restrain the public appetite for war.

Chalmers's treatment here, of course, suffered from many weaknesses. He neglected to consider the effect of taxation on prices. He over-simplified and exaggerated the net social burden of public debts by saying that the present burden would be equally heavy under both the tax and the loan alternatives. He was, indeed, viewing society as a homogeneous whole and ignoring the fact that liquid funds could be raised with much less sacrifice of utility at the time by means of a loan from the richer classes than by means of a taxation spread over the whole community. (There is, however, a large element of truth in Chalmers's view that it was government expenditure, which, by taking up the slack, by bringing about a state of fuller employment and by occasioning higher prices, created the wealth thus contributed by loans.) Moreover, Chalmers did not comprehend the multiplicity of the variables which determined the size of the future debt burden, and concluded that it was equal to the present burden (which, in turn was the same whatever method of extraordinary finance was adopted).

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[1] *Works*, Vol. XIX, p. 141.
[2] *Ibid.*, Vol. XX, pp. 71 ff.
[3] *Ibid.*, p. 71, footnote.
[4] Cf. *Ibid.*, pp. 72–73.
[5] *Ibid.*, pp. 73 ff.
[6] *Ibid.*, Vol. XX, pp. 73–74.
[7] *Ibid.*, p. 74.
[8] *Ibid.*, p. 75.
[9] *Ibid.*, p. 75.
[10] *Ibid.*, p. 76.
[11] *Ibid.*, p. 76.

The substitution of finance " by the pure operation of credit "—*i.e.*, by a government issue of money—for that by borrowing would not in Chalmers's opinion alter the nature of the transaction. There was " an increase of prices . . . if not by a diminution of the money that formerly went to the side of production, at least by an increase of the money that now goes to the side of demand ".[1] Moreover, there was the possibility that the capitalists, with what capital they had left after contributing to the government loans, " may not confine their operations to what they could barely do with their own proper capital. They may superadd the power of credit to that of capital, which, in the circumstances supposed they can do with all the greater safety, because of the large return . . . that is now awaiting them ".[2] There was nothing in borrowing itself which tended to raise factorial prices (the additional demands of the government for labour, etc., being neutralised by the proportionally smaller demand of the capitalists); the utilisation of credit by the entrepreneurs would lead to a rise in the prices of factors of production, and chiefly in the money price of labour.[3] A rise in wages would, in turn, lead by the force of competition, to a rise in the prices of all that labourers purchased, and particularly to a rise in the money price of grain.[4]

During a period of high prices and profits resulting from public borrowing, " it is by the mere annuitants of all classes that the pressure is chiefly felt—who draw their stated income, and, with the same nominal revenue as before, have to encounter that excess of prices which we ascribe to the operation of the funding system "[5] In the light of this phrase, " annuitants of all classes " and, indeed, in the light of the whole of this Note D, it is clear that Chalmers did not intend to convey the impression that public borrowing weighed particularly heavily on the labourers. Though Chalmers described public loans as abstractions of capital from productive employment and though he adhered to a wage-fund doctrine (linked to a theory that population, unless the moral check was strongly operative, always tended to increase up to the limit of bare subsistence), he was well aware of the fact that the labourers thrown out of employment would be re-employed by the government.

In a Postscript to *The Supreme Importance of a Right Moral to a Right Economical State of the Community* (1832), Chalmers adverted to his theory of the re-creation of capital after the abstractions of public loans and elaborated again in regard to the distinction between circulating and fixed capital: " Our illustrations certainly are taken more from the case of circulating, than of fixed, capital; but the reasoning is equally solid or equally fallacious with both. Neither the fixed nor the circulating capital can be extended beyond the limits of that returning power, by which they are returned with a profit".[6] He was by this date quite emphatic that "higher prices do, themselves, explain the resuscitation of capital ".[7]

II

J. R. McCulloch criticising Chalmers's theory, considered that nothing could be " more utterly at variance with principle and experience, than the notion that the destruction of capital, by waste or otherwise, is immediately made good through a rise of prices ", and he believed, " that the waste of capital will never be repaired except by an increase in productive power, or by a greater degree of economy, or both ".[8] McCulloch appears to have pronounced a further weighty criticism of Chalmers's theory, when he pointed out the fact, ignored by that theory, that it was " the stupendous inventions and discoveries of Watt, Arkwright and Wedgwood " which provided the true basis of the increased productive capacity and national capital during the Napoleonic Wars. When, however, he stated that: " To the circumstances mentioned by Dr. Chalmers we owe absolutely nothing ",[9] McCulloch failed to recognise the element of truth in Chalmers's theory that public expenditure inflates the national liquid money-capital and thus, to some extent, provides the wherewithal for its own finance.[10]

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[1] *Works*, p. 79.
[2] *Ibid.*, p. 80.
[3] *Ibid.*, pp. 81–82.
[4] *Ibid.*, p. 82.
[5] *Ibid.*, p. 87.
[6] Loc. cit., p. 230.
[7] *Ibid.*, p. 231.
[8] " Dr. Chalmers on Political Economy ", *Edinburgh Review*, October, 1832, p. 67.
[9] *Ibid.*, p. 68.
[10] Cf. Hicks and Rostas, *The Taxation of War Wealth* (1941), Part I.

John Stuart Mill adverted to some of Chalmers's ideas in his *Principles of Political Economy*, first published in 1848. He regarded the state of affairs where, " the whole amount borrowed and destroyed by the government was abstracted by the lender from productive employment as the most unsatisfactory case possible ",[1] but he found it useful to consider this case and seemed to consider all the other possibilities in relation to it. Mill agreed with Chalmers that periods when extraordinarily large governmental expenditure was financed by public loans had often been periods of apparent prosperity. Whereas, however, it was Chalmers's view that this prosperity was made possible by the restoration of capital from the privation of the general body of consumers, Mill held that: " The breach made in the capital of the country is instantly repaired by the privations and often the real misery of the working class ".[2] He stated that since a public loan could not have been taken from " tools, machinery and buildings " it " must have been wholly drawn from the portion employed in paying labourers ".[3] The wage-fund doctrine was thus utilised by Mill to show that " if the labourers produce as much as usual, having being paid less by so many millions sterling, these millions are gained by their employers ",[4] so that prosperity falls to the lot of " those whose prosperity usually passes in the estimation of society for national prosperity ".[5]

Mill, then, without any of the qualifications indicated in the above-mentioned theories of Chalmers, proceeded to attribute to the latter this, his own view, of a special case.[6] In fact, however, this theory of an exceptional burden on the labouring classes appears to have been nothing more and nothing less than Mill's own view of " the most unfavourable case possible " of public loans. Borrowing in such a case was " not a substitute for raising the supplies within the year " but merely a device for taxing exclusively the working classes. Moreover, since it was the employers rather than the State who benefited from such a system, and since the system would also leave a heritage in the form of a perpetual interest charge, public borrowing in such circumstances might " be pronounced the very worst which, in the present state of civilisation is still included in the catalogue of financial expedients ".[7]

Following Mill, Bastable and Edgeworth accepted Chalmers's parentage of this rigid theory. C. F. Bastable ascribed to Chalmers the view that public borrowing was particularly " disadvantageous and oppressive to the labourers ", and that " by taxation the charge is spread over the whole revenue of the society, while by borrowing it is placed on that part of circulating capital that goes to reward the labourers, and, as a necessary consequence, lowers their remuneration to the benefit of the capitalists ".[8] He cogently observed that this theory was tied up with the most rigid version of the wage-fund doctrine and the slightest refutation of that doctrine would damage the public debt theory derived from it. Moreover, loans did not come from the portion of the country's capital which would be applied only to the maintenance of labour but " from floating capital that would otherwise have been applied to fixed capital, raw material or payment of workers in proportion determined by the actual circumstances ".[9]

F. Y. Edgeworth in his *The Cost of the War: A Lecture* (1915) on the whole endorsed Mill's view of Chalmers's theory.[10] Chalmers Edgeworth criticised because " like others of the older writers, or more than others, he exaggerates the superiority of taxation over an internal war loan ".[11] He humorously stated regarding Chalmers that: " His view of a public debt as a double burden is twice removed from the view which I have entertained ".[12]

There are also two paragraphs dealing with Chalmers's theory in S. Matsushita's *The Economic Effects of Public Debts* (New York, 1932), pp. 31–32. In his text Matsushita referred to Chalmers's *Political Economy* (1832), Vol. 11, pp. 71–78, and in his bibliography to Chalmers's *Political Economy* (New York, 1832). He went even further than Mill and Bastable in attributing to Chalmers in 1832 a theory based upon a rigid wage-fund doctrine.

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[1] *Principles*, ed. Ashley, p. 76.

[2] *Ibid.*, p. 76.

[3] *Ibid.*, p. 76.

[4] *Ibid.*, p. 77.

[5] *Ibid.*, p. 77. Cf. Wicksell, *Finanztheoretische Untersuchungen* (1896), pp. 139–140, for an account of the application of the wage fund doctrine to public debt theory in the work of Wagner.

[6] Cf. *Ibid.*, pp. 77, footnote, and 78.

[7] *Ibid.*, p. 873.

[8] *Public Finance*, 3rd edition, 1903, p. 665.

[9] *Ibid.*, p. 667.

[10] Op. cit., p. 46.

[11] *Ibid.*, p. 23.

[12] *Ibid.*, p. 45.

III

The only explanation that it seems possible to offer regarding this continued misapprehension of Chalmers's theory is that the later theorists followed Mill's interpretation; it seems fairly obvious that Bastable, Edgeworth and Matsushita knew their Mill better than their Chalmers. Moreover, the views of both Mill and Edgeworth seem to have hinged on Chalmers's early writings, which mention the restoration of capital by the fall in wages, rather than on his more mature works of 1832; this is certainly suggested by Edgeworth's references[1] to Chalmers's *Extent and Stability of National Resources* of 1808.

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[1] Op. cit., p. 46.

Book Reviews

The Economic Lessons of the Nineteen-Thirties. By H. W. ARNDT. Oxford University Press. 1944. 314 pp. 12s. 6d.

This book, though arising out of the work of a Chatham House committee, and issued under Chatham House auspices, is not an agreed report. We are indeed not told the names of the members of the group concerned, except for those of Sir Andrew McFadyean, whose trenchant note of dissent is published as an appendix, and Professor A. G. B. Fisher, who concurs with him. The report, therefore, stands in the name of Mr. H. W. Arndt, the research assistant to the group, to whom the work of drafting was entrusted, and with all respect to him can hardly carry the weight of a normal Chatham House report.

The report contains a brief summary of economic developments up to 1929, a fuller description of events in the United States, Great Britain, France, Germany and Sweden in the years 1930–39, a discussion of international action in the economic field in those and earlier years, and a final chapter of diagnosis and recommendations. If allowance is made for the obvious predilections of the author, which, in spite of his valiant attempts at impartiality, continually affect the tone of his comments, the descriptive sections of the book provide a clear and often acute outline of many of the main events of recent economic history. With his picture of a world with limited and decreasing powers of adaptation to change, restored after the last war to no more than a partial and unstable equilibrium, and thrown into desperate confusion by the onset of a severe cyclical depression in the United States, few will disagree, any more than they will with his verdict on most of the unco-ordinated and usually self- or mutually frustrating efforts of individual governments to remedy the positions of their own countries. Nor is there a great deal to quarrel with, except on grounds of inadequacy, in some of his recommendations for the future. His proposals for mitigating the effects of cyclical depressions go scarcely further than the British Government's White Paper, while his suggestions for curbing the power of producers' monopolies (labour monopolies are hardly mentioned) are tentative in the extreme. Even his contention that some (not very clearly specified) form of international control of foreign exchanges, loans and trade will be necessary during the interim period of post-war reconstruction will not arouse much opposition, in spite of doubts about the political difficulties involved.

It is indeed only in the last two or three pages of his report that Mr. Arndt's proposals (in contrast to his comments) become really controversial. Here he contends that the economic and social frictions generated by attempts to adjust the internal economic structures of individual countries to the changing requirements of a world price economy are, and henceforward always will be, so great that never again must countries allow their economies to be exposed to the effects of an automatic international monetary system; and that, if they should ever make the attempt, the result would inevitably be a new collapse which would compel the resumption of controls. He therefore argues that " the obvious, and the only, answer to the dilemma is that there must indeed be planning of international trade and investment, and that there may even be ' discrimination ', but that planning must be international wherever possible . . . There will have to be direct planning of international economic intercourse. How far this will have to take the form of international co-operation, and how far it will be possible to get countries to submit to supra-national authorities—these are important but secondary questions, provided the imperative need for planned international action is recognised and acknowledged ".

I do not think that Mr. Arndt can be very familiar with the working of the machinery of economic planning, even under the unifying pressure of a common enemy, or he would hardly regard these questions as secondary. In some circumstances a human pilot in an aircraft may be preferable to an automatic pilot. But if the choice is between an automatic pilot and a dozen human pilots, all with different views as to the correct course and all wrestling for the controls, there can be little doubt which the passengers would prefer !

Surely the alternative policy, of taking all possible steps to restore world economic equilibrium during the period of reconstruction, and subsequently of using the full powers of governments to facilitate their peoples' adjustments to the impartial forces of an international price system, is both more practicable and more desirable. It may well be true that after its previous accident our world was both inadequately treated and discharged from hospital prematurely; but this is hardly an argument for keeping it this time in splints for the rest of its life.

F. W. PAISH.

The Control of Germany and Japan. By HAROLD G. MOULTON and LOUIS MARLIO. Brookings Institution, Washington, D.C., and Faber and Faber, London. 1945. 116 pp. 10s.

Among the long list of post-war problems pressed upon our attention to-day, few have aroused so much feverish interest as the control of the industrial potential of Germany and Japan. The disappointing experience of the disarmament policy formally adopted after 1918 has, in many minds, demonstrated the necessity for more radical and more direct action impinging upon some or all of those sectors of the German and Japanese economies whose unfettered expansion during the inter-war period made possible the historical process of rearmament. Messrs. Moulton and Marlio have made a brief but succinct analysis of most of the proposals which have attracted attention as likely to be effective for this purpose. For the most part the results are discouraging to ardent hopes that by such means peace may be permanently assured for the world, and they conclude that " only *military* force can be relied upon to give complete protection against nations bent on aggression ". They lay down two guiding principles which must be observed if economic control measures are to be effective. " First, the economic devices must not be permitted to throttle the economic life of the country against which they are imposed ", and " second, the economic control measures selected must be administratively feasible—relatively easy to enforce. A large army of foreign policing agents is a source of continuous humiliation and friction; hence the control measure should be of a character which requires a relatively small number of controllers ".

Judged by reference to these principles, most of the current popular solutions are shown to be defective, and are therefore rejected. Territorial adjustments are condemned, because, unless they destroyed the economic interdependence of the various regions of the Reich, they would be ineffective, while if this interdependence were destroyed, they would have such destructive repercussions, upon both Europe and the world as a whole, that the policy would be self-defeating. Similarly plans designed to destroy once for all the industrialism upon which war power rests would involve costs, for other countries as well as for Germany, so high as to be prohibitive. " Any general system of economic control, to be genuinely effective, would have to be so comprehensive in scope and so disruptive economically that it would undermine world economic prosperity and thus work against enduring peace ". Partial controls, involving the allocation of strategic minerals, are also rejected as impracticable, on account of " the broadly distributed production of minerals, the ease with which they may be smuggled in small consignments and the possibilities of substitution, especially in the light of continuing technical developments ". Direct controls over selected key industries are regarded more favourably, and especially the prohibition of the production of aluminium ingots, supervision of the electric-power industry, and control of civil aviation, but even these, it is argued, would afford no watertight guarantees against the risks of future technological change, of secret stockpiling, and of the development of war facilities in neighbouring friendly countries.

The future historian may indeed well conclude that much of the elaborate technical discussion which has been devoted to this problem has in fact evaded the main issue, and therefore for the most part been a waste of time. The real lesson to be learnt from the experience of the inter-war period is not that the technical disarmament devices then formally approved were defective, but that there was insufficient harmony between the great powers who for the time being had the fate of the world in their hands to ensure either that the technical devices were consistently applied, or, more important, that vigorous steps were taken to lay the foundations for a more stable social and economic world structure. The substitution of more drastic and far-reaching technical devices for those which were approved after 1918 would still, in the absence of such harmony, fail to produce satisfactory results. The more radical controls now so widely commended are offered to us in effect as a re-insurance against the risk that this essential harmony may again be lacking. In that event, however, it will also again be impossible to maintain the controls, while if such harmony is assured, the proposed controls will be unnecessary. In certain circumstances re-insurance is an obviously prudent policy, but there is nothing to be said for it if the realisation of that very risk against which re-insurance is being sought,—in this case, failure to maintain close and friendly relations between the leaders of the United Nations,—must at the same time inevitably render worthless the assets from which it is hoped that the re-insurance policy may eventually be paid. It may be confidently anticipated that as time goes on our minds will be more and more troubled by this insoluble dilemma, but there is unfortunately a grave risk that post-war policy may go sufficiently far in the direction of ineffective control to ensure that the world will have to suffer the consequence of a considerable part of the economic loss involved, before the inherent futility of action of this kind is generally appreciated.

The discussion of the German case is followed in this book by a similar analysis applied to Japan, though it might have been supposed that, if effective control of Germany could be assumed, there was little pressing necessity to apply similar measures to Japan. Japan is obviously in a weaker position than Germany, much poorer in strategic war materials and more heavily dependent on foreign trade, and as it is argued that the importance for Japanese war plans of her colonies and other annexed areas has been based at least as much upon the industrial developments which have occurred there, and especially in Manchuria and Korea, as upon their value as sources of supply for Japan itself,—with the possible exception of sugar from Formosa,—the conclusion would seem to follow that, if Japan is deprived of these territories, in accordance with the Cairo Declaration of December, 1943, she could not again become a strong military power. Messrs. Moulton and Marlio do not, however, accept the view that colonial dispossession will be enough. The same factors which make Japan more vulnerable than Germany also diminish some of the technical difficulties of plans for controlling key industries or the importation of raw materials, and they therefore incline to the view that these might advantageously be considered as supplementary measures.

The positive programme outlined in this book contemplates the termination of military occupation at a relatively early date, to be followed by the creation of a Rearmament Detection and Prevention Board, composed of a small number of military officials, with power of independent action within the scope of its delegated authority, and authorised to maintain a number of supervisors sufficient to detect any evasions of the disarmament provisions, to call the attention of the German or Japanese government to any rearmament preparations which might be discovered, and if such warnings were not promptly heeded, to apply the necessary coercive measures. This administration, it is argued, would have a better chance of success if it were kept separate from the development of a general association of nations.

Messrs. Moulton and Marlio do not press their scepticism about economic controls to the point of rejecting them altogether. Certain economic devices, they think, might usefully supplement a system of military controls. It remains a matter of some doubt whether we shall not find in the real post-war world that deference to current fashions of thought even to this modest extent may carry us too far and add unnecessarily and unhelpfully to the inevitably overwhelming complications of our post-war problems. The argument is, however, presented here in a spirit of sober realism, and it would be an understatement to say that the risks which its authors run of being embarrassed some twenty years hence,—if they survive so long,—by a reminder of what they wrote on this subject during the war are much less than the corresponding risks run by many of the more enthusiastic and romantic writers who have speculated in this field.

ALLAN G. B. FISHER.

National Health Insurance: A Critical Study. By HERMANN LEVY. National Institute of Economic and Social Research. Economic and Social Studies IV. Cambridge University Press. 1944. x + 366 pp. 18s.

Owing to the interval which at present elapses between the completion of a book and the date of its publication, Professor Levy's study of National Health Insurance is published at a time when the old system is due for a thorough overhaul. His manuscript was substantially completed before the appearance of the Beveridge Report, and there is no mention of the recent White Paper on the National Health

Service. If the government carry out their declared intentions, the book will therefore mainly be of historical interest.

The British system of health insurance has been attacked and criticised on many counts: it does not cover all those in need of health insurance, the services provided are inadequate, the benefits too low and unequal, and there is insufficient incentive for the doctor to give his best when faced with the conflicting demands of his private and his panel patients. The system of administration through Approved Societies is clumsy and inefficient.

Professor Levy makes all these criticisms and he gives chapter and verse for each of them. He shows how approved societies, which were permitted to have a share in the administration of the scheme as a bribe for their support, have proved a vested interest opposed to any change, and how the system of finance that has been adopted makes any extension of benefits very difficult. On the medical side he points out the limitations of the insurance service and gives a valuable account of the relations between the insurance doctor, the hospital and the local government medical service. He is particularly caustic about the provision of surgical appliances and artificial limbs, for the provision of which many insured persons have to rely on charitable societies. From the report of the Surgical Aid Society he quotes a case where the patient had to wait for an artificial limb for as long as 15 years.

But although the greater part of the book is devoted to criticisms of the system (and rightly so), the conclusions and recommendations are anything but revolutionary. Approved Societies, it is true, are to be abolished (it would indeed be hard to find a disinterested observer with a kind word to say for them), but it is proposed to substitute a territorial or occupational system of insurance funds on the German model in their place. Health Insurance is to be more closely linked to Workmen's Compensation and the scope of industrial insurance is to be severely curtailed. But the author believes " that National Health Insurance can and should remain a separate statutory social service under an entirely different system of organisation and administration ".

It is difficult to agree with Professor Levy's conclusions. From a study of the book it would appear that one of the main causes of the failure of National Health Insurance was that it was regarded as an *insurance* scheme rather than a social service. Any extension of the scheme, any increase in benefits would have to be paid for by increased premiums. Although ideas of what is an adequate standard of medical treatment have changed, National Health Insurance benefits still stand largely where they were when the scheme was first designed in 1911. Additional medical services have been provided by local authorities, or left to the voluntary hospital, and whilst the ordinary insured person was frequently, though by no means always, able to obtain very good *medical* treatment at a comparatively low cost, far too little attention was paid to his needs during the period of convalescence or rehabilitation when any sickness or disability benefit paid to him bore no relation whatsoever to his needs.

If the standard of national health is to be improved (and a good deal of scope still exists for its improvement) the conclusion seems inevitable that a completely integrated National Health Service is a necessity and that such a service should be regarded as a social service rather than as a mass insurance scheme in which benefits and premiums are strictly related one to another in an actuarial fashion. National Health Insurance is a very special kind of insurance in that the same premium is quoted to everyone irrespective of his risks, and that differences in benefits are due merely to the accident of choosing a particular Approved Society. The present contribution is therefore not so much an actuarial premium as a tax on those working in an occupation in which the incidence of sickness is low. Moreover, this tax can be easily evaded if the insured person is willing to choose his Approved Society with some care, for some of them refuse to accept members in occupations in which there is a high sickness rate. It would surely be more equitable to realise that national health is of interest to all members of the community and that a nationally integrated health service paying adequate benefits and dealing with all aspects of sickness, medical and social, is a logical extension of our present public health services, and like them properly financed out of general taxation.

Some of Professor Levy's proposals, *e.g.*, the abolition of Approved Societies, now form part of the government programme, others have been rejected. But even if the new service is radically different in outline from what Professor Levy would like to see, his study is invaluable in pointing out the pitfalls and mistakes to be avoided when the new scheme is built up.

E. GREBENIK.

The Danube Basin and the German Economic Sphere. By ANTONIN BASCH. The International Library of Sociology and Social Reconstruction. Editor: Karl Mannheim. Kegan Paul, Trench, Trubner and Co., Ltd. 1944. xiv + 272 pp. 18s.

This is a book of undoubted scholarly quality. Not only is it well documented from printed sources, but the author, who has held for many years important positions in economic affairs in Czechoslovakia, writes with the advantage of having been a close observer of, and, to some extent, a participant in the shaping of, the development which he describes.

The book deals with a chapter in recent economic history which holds lessons of vital importance for future policy. Unfortunately, points of major and fundamental importance do not always stand out against details with the necessary clearness and conciseness, and frequent repetitions are rather irritating to the reader.

The main theme is, first, the fight of the countries in South Eastern Europe (including Czechoslovakia and Austria) against the impact of the depression of 1930 on their economic and financial systems, and, second, the situation and measures which made it possible for Germany to obtain a decisive, if varying, hold on the economic and consequently also the political life of these countries. The story is told with restraint and objectivity. Nevertheless, one cannot help feeling that the picture is not quite balanced. The author leaves no doubt of his belief that the German ascendancy was first assisted, and later made inevitable, by the denial of international help to the countries and especially by the difficulties which they encountered on the " free " markets of Western Europe in selling their agricultural produce. However, these assertions would carry more conviction if the nature of the obstacles had been described in greater detail. Thus, although the importance of easy access to the British market is emphasised, it is rather startling to find the Ottawa Agreements not even mentioned. There can be, however, little doubt that they prejudiced, in addition to other difficulties, the position of important exports from the Balkans—Greece and Bulgaria in particular—in Britain. Very little is said about French policy which obstructed throughout the inter-war period any move towards greater unity of action in the region.

These omissions are serious since, as the author rightly points out, a more prosperous and peaceful future in the Danube Basin depends largely on whether its exports will find easier conditions than before the present war in the markets of the highly industrialised countries of Western Europe.

In a programme of internal development for the region, Dr. Basch recommends in the first place increased diversification and standardisation of agricultural production, and, further, a certain amount of industrial development, both with the help of foreign capital. Emigration, which played such a large part before 1914 in easing economic and social tensions, is not among the measures suggested for relieving pressure on the land. There can be, however, no doubt that a certain amount of emigration would facilitate the policy of development suggested, and it is likewise an undeniable fact that the impasse which was reached in 1930 was largely due to the closing of this outlet, particularly in the United States. Emigration may not be an ideal and is certainly not a final remedy against a low standard of living, but it is somewhat disappointing that the author is willing to accept as an unalterable fact that emigration to oversea on a large scale cannot be resumed, without even discussing the issue. If it is true, " that any solution of the problems of central and eastern Europe must be found in a setting wider than Europe ", as Professor Condliffe points out in his stimulating Introduction, the restoration of freedom of international migration will be no less a pre-requisite for such a solution than the return to multilateral trade.

If one is inclined to entertain some doubts as to the practicability and compatibility of some of the measures outlined in the last chapters, the facts assembled in the book should prove of considerable usefulness when plans are drawn up for the economic reconstruction and development of the Danube Basin.

CHARLOTTE LEUBUSCHER.

National Income and Expenditure. By J. E. MEADE and RICHARD STONE. Oxford University Press. London: Humphrey Milford. 1944. 36 pp. 2s.

The aim of this booklet is to explain the official estimates of the national income to the layman. The approach follows the now familiar lines of " social accounting ", stressing the arbitrariness of the concept of national income. It is, in many ways, a pity that the authors do not emphasise, as Marshall or Pigou do, that the national income is something real, money being merely the common denominator. The wartime rise in the national income is analysed (p. 26) in terms of an increase in activity and an increase in wage-rates (which is, in any case, a short-period analysis), instead of in terms of output and prices. Nor do the authors describe the contents of the national income (that housewives' services are excluded, payments in kind included, and so on), in which field there is a well-established British practice, and the emphasis of which is important when considering wartime changes.

The main drawback is that the authors assume that the reader knows what income, in the economic sense, is, and there is no attempt to develop a definition of the national income from the central concept of income. Five definitions are distinguished (it being implicitly assumed that real and not money capital is maintained intact): the total of personal incomes, the total of private incomes (personal and impersonal), net national income at factor cost, net national income at market prices, and gross national income at market prices. (Why no gross national income at factor cost ?) It is difficult to see why the first two of these should be regarded as alternative definitions of the national income, when in fact they can be called by a different name, and do differ from the national income in any accepted sense of the term.

The division of private incomes into personal and impersonal income is altogether not a happy one. Income-tax law does make a distinction between personal and non-personal incomes, since the latter is always taxed at the standard rate, and is not liable to surtax. The official definition includes in impersonal income the undistributed profits of companies and also (unlike the Inland Revenue) that of individual traders or farmers. On the other hand, the official definition excludes the investment income of non-profit-making bodies, and of life insurance funds. Thus we arrive at the anomalous situation that the income of a farmer, which he does not spend, might be regarded as impersonal income, whilst the investment income of the Church of England is included in personal income.

The authors give, on pp. 12–13, different meaning to the terms "outlay" and "expenditure". These terms have so far been used as synonyms, and it is doubtful whether the distinction will go into common use.

The U.S. national income figures, which are used for comparison, are rounded off to the nearest $500 mn., since their definition is not exactly the same as the British. But should not the lay reader be warned not to regard the British figures as being accurate to the nearest £1 mn. ?

T. BARNA.

Comparative Operating Experience of Consumer Instalment Financing Agencies and Commercial Banks, 1929–41. By ERNST A. DAUER. National Bureau of Economic Research. 1944. xviii + 221 pp. $3.00.

Credit for the "small man" in the United States is provided by five different types of specialised agency, as well as by the commercial banks, wholesalers, and so forth. Sales Finance Companies are primarily engaged in discounting hire-purchase contracts; the remainder extend direct cash loans to borrowers. Personal Finance Companies make small loans to individuals, subject to special money-lending legislation in most States. Industrial banks, of two kinds, make advances to small businesses, the "investment" type securing its assets and doubling the effective rate of interest earned by coupling with the advance a separate contract for repayment, by the deposit of instalments which include interest throughout on the whole amount outstanding. Credit Unions, which are mutual associations, offer similar facilities to their own members only. The combined agencies were in 1941 providing credit to the extent of some six billion dollars.

The National Bureau of Economic Research has undertaken a series of studies into the nature and effects of consumer finance, of which the present volume is No. 10. It combines information derived from several different sources. The Income Tax Study of the Works Progress Administration, in association with the U.S.A. Treasury, provided an analysis of the Federal Income Tax returns for some 600 companies in each of the three years 1929, 1933, 1936. The National Credit Office, Inc., made available for tabulation the principal items in some 30–50 companies' annual statements for the whole or part of the 13 years covered. The F.D.I.C. analysed the financial returns for 60–70 industrial banks, and provided comparable data covering some thousands of commercial banks. Finally, the Farm Credit Association made available figures for the Federal Credit Unions for the last seven years of the period, during which time they increased in numbers from 762 to 4,144.

The task undertaken in this volume is to combine the figures so furnished into a comparative study, deducing therefrom factual conclusions on the sources and uses of the funds available, the sources and rates of gross income derived, costs incurred, and profits realised. The work has been exhaustively done, and constitutes a mine of information about comparative results in a sphere of financial activity which has been little studied because of the small size of individual transactions.

As a contribution to our knowledge of the American capital market " set-up " the volume will certainly have a use; but it is hard to see what real economic value there is in all its elaboration. This consideration will tend to inflame the uncharitable feelings which, both as a specimen of lavish book-production, and as an example of the possible achievements of economic research sponsored by organisations with really adequate resources, the volume will arouse in the breasts of students in this country, whose search after knowledge could be so much more effective if only finance, the Paper Controller, and the Ministry of Labour would permit.

J. K. Horsefield.

Theoretische Grundlagen der Wirtschaftspolitik. Band I. By Walter Adolf Jöhr. (St. Galler Wirtschaftswissenschaftliche Forschungen. Band 1.) Verlag der Fehr'schen Buchhandlung. St. Gallen. 1943. xv + 267 pp.

Dr. Jöhr, lecturer at the Handels-Hochschule, St. Gallen, Switzerland, has set himself the rather ambitious task of working out a theoretical system of economic policy. In the present book, volume I, he endeavours to lay the foundation of such a system by examining " the arguments for economic freedom " and " the model of perfect competition and its proximity to reality ".

His approach consists essentially in contrasting the theoretical model of a market economy (what he calls " Katallaktik ") with the " real World ". He enumerates four factors as being mainly responsible for this contrast: monopoly, frictions, paralysis of the profit impulse, and power. In his view the existence of this contrast makes a case " against economic Liberalism, but neither against all economic freedom nor against a competitive market economy " (264). It would seem that " economic Liberalism ", in this context, means *laissez faire*. Unfortunately, Dr. Jöhr rests content with this negative conclusion and fails to tell us how he proposes to protect economic freedom and a competitive market economy against the effects of the four disturbing factors mentioned. We cannot even be sure that this is his ultimate aim. But we are promised a second volume which will also deal with dynamic problems.

One cannot help feeling that for a book of 267 pages, printed in the grand pre-War manner, this is a somewhat meagre result. Yet, ours is a most imperfect world and we have all had to learn patience. That Dr. Jöhr, who ten years ago expounded the idea of a Corporative Economy should, by 1943, have come to realise some of the manifold benefits of a competitive market economy, is certainly encouraging. Or would it be too bold to say that it is a sign of the times ?

L. M. Lachmann.

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Profit: Accounting Theory and Economics

By Harry Norris

The purpose of this article is to examine the accounting notion of " profit ", with passing reference to " income ". It is hoped that this tentative statement of some of the principles underlying business accounts will serve to put economists in a position to pass judgment on these principles, and perhaps to revise their own ideas of the nature of the concepts " profit " and " income ".

It has long been a cause for concern to me that economists and accountants (I happen to be one of the latter) have been discussing and measuring profits and income for a very long time and have nevertheless failed to reach any close agreement as to the nature of the things they are dealing in and the methods appropriate to their measurement. It is not so much that they have disagreed heartily—that might have produced some healthy controversy, the two professions simply have not been in contact. As they deal with similar things, though naturally with different objects in view, this lack of mutual aid—for that is what an exchange of opinions would amount to—is not only extraordinary, it is reprehensible.

I have to admit, however, that economists have taken a little more notice of us than we have of them. Since they use the accounts we prepare, it is but natural that they should have some interest in their mode of construction, and it is possible to find some comment on accounting practice. Thus we have Professor Irving Fisher's work, *The Nature of Capital and Income*, and Professor J. B. Canning's *The Economics of Accountancy*. More recently, there has been a spate of articles and footnotes on the question of the valuation of business holdings of stocks of goods. From these comments it is possible to piece together something of what economists think of accountants—professionally, that is. It is also possible to gain from this literature some idea of the view economists take of the nature of income. As an accountant, I am surprised to find that this view—or rather, these views—are much vaguer than I might have expected them to be. There is undoubtedly a tendency to criticise the accountant's idea of profit—particularly in relation to notions of maintenance of capital; but I am not left with the impression of any generally accepted notion of profit amongst economists. Such consensus as exists seems to me to be open to criticism, and whilst I am not in full agreement with the whole of present-day accounting theory and practice (quite apart from inadequate disclosure of company results—a regrettable feature which few accountants view with equanimity), I would, on the whole, uphold it against the views which have been put forward by economists.

With the most extensive study so far made by any economist in the field of income, that of Irving Fisher, I am in the strongest disagreement. Not only are his ideas of accepted practice in accountancy largely misconceived, but his analysis of the nature of income seems to me to be open to grave objections. As is well known, his view of income is that it is the services yielded to an individual by the use of his capital. From the example given at page 135 of *The Nature of Capital and Income* it is clear that he considers income to be equal to personal expenditure plus value of service rendered by durable consumption goods. Thus a man who earns a salary of £1,000 in a year, invests £200 and spends the remainder on living expenses, has an income, on Fisher's view, of £800. (The details given of the " lawyer's " affairs are a little more involved than this, but that is what they amount to.) In relation to accountancy practice a few extracts from the book should be sufficient:

> " Realised income is the value of the actual services secured from the capital; earned income is found by adding to realised income the increase of capital value, or deducting from it the decrease. We may designate them simply as income and earnings " (p. 234).
>
> " It is . . . to earnings that accountants instinctively give their main attention. But they err grievously when they attempt to spirit away realised income and put earned income in its place . . . earnings cannot be calculated except by the aid of depreciation, depreciation cannot be calculated except from capital-value, and capital-value cannot be calculated except from expected realised income " (p. 235).

> " But so persistent is the accountant's instinct to put aside realised income in favour of earnings that we need to point out in detail the confusions which arise, unless income and earnings are carefully distinguished " (p. 236).
>
> ". . . the fallacy consists in reckoning depreciation of capital as part of outgo, or appreciation of capital as a part of income. This usage is difficult to combat, for with many it has become habitual " (p. 238).

This is rather a staggering indictment, for not only is this " usage " not " habitual ", it is never employed at all. In fact, economists' usual criticisms of accounting practice are that it takes no account of maintenance of capital, that an accretion in the value of the business is as much profit as surplus sale proceeds, but that the accountant illogically will not act on this basis. However, leaving over the rights and wrongs of adjusting for value fluctuations, the accountant certainly does not add increase of capital value of a business or of particular assets in arriving at a profit figure.

It is, perhaps, not necessary to deal with Fisher's views at greater length, for there is no evidence that his idea of the nature of income is widely held amongst economists. His work is worth mentioning principally because it is the most exhaustive examination which has been undertaken by any economist (so far as I am aware) and because it contains the most precisely stated idea of income. If we turn to more recent works by economists, we find that Professor J. R. Hicks, in *Value and Capital*, is reluctant to define income. In Chapter XIV on " Income " (p. 171 *et seq.*), he discounts the term's usefulness in economic analysis. In fact, he goes further and considers " the concept of income to be one which the positive theoretical economist only employs in his arguments at his peril. For him, income is a very dangerous term, and it can be avoided . . ." (p. 180). His view is that there does not exist any logical category to fit this word " income ", and that all that is needed is some rough-and-ready practical notion related to " living within one's income ". His doubts appear to centre at the difficulty, or perhaps the impossibility, of finding a usable measure which will fit in with the " central meaning " of income. As to this " central meaning ", he has apparently no doubt that income is " the maximum value which he can consume during a week, and still expect to be as well off at the end of the week as he was at the beginning " (p. 172). This, of course, is defensible as a mode of viewing " income ", but it is not the view which is held by accountants, nor, I imagine, by the average proprietor of a business. To these, current revenue yield is distinguishable enough to be independent of comparisons of expectations at successive dates: in other words, considerations of the maintenance of a standard of " well-offness " do not enter into the accounting measurement of the income of a proprietor from his business.[1] Current yield may have declined, suggesting a decline of value and of future prospects of revenues; but it is current yield which remains the concern's profit irrespective of this " depreciation ", and for this reason, namely that this conception *is used* by the business world, it is, I should have thought, a highly significant notion in economic analysis.

Hicks refers to some possible approximations to his " central meaning " of the term " income ", and as they are probably ideas which would be upheld by many economists, it will clarify the position if I summarise them:—

(1) Income is the residuum of revenue after setting aside funds to maintain capital value at a constant figure. Thus, if a business is worth £10,000 on January 1st and £9,800 on December 31st and the owner has had £500 in cash from it during the year, then his income from it is £300.

(2) Income is the residuum of revenue after setting aside funds to maintain future expectations of income at the same figure.

(3) Similar to (2), substituting " expectations of purchasing power of future income " for " expectations of income ".

There is some difference of opinion on the question of maintenance of capital, principally as to whether this should be capital value or " real " capital. Thus we find Kaldor and Barna suggesting that stocks should not be valued at current prices, implying, presumably, that they should be charged, as used, at their replacement cost. Valuation at recent cost they describe as:—

> " logically unsatisfactory, since it corresponds neither to the definition of income as the ' amount available after maintaining the *real amount* of capital

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[1] I shall draw some distinctions from this view in considering incomes from marketable securities, but this sharp difference between the views of economists, as interpreted by Professor Hicks, and accountants remains without qualification in relation to income from the " privately-owned " business.

> assets intact ', nor to the definition of income as the ' amount available after maintaining the money value of capital assets constant ', being a composite mixture of both. It follows the first definition as regards fixed capital and the second definition as regards circulating capital."[1]

I feel sure accountants would not agree that there existed any discrepancy in the accounting treatment of these two classes of assets except as regards the complicating factor of market value—which remains a matter of detail. The principle is a uniform one—to take in the asset at its cost price. Nor do I agree that a revaluation of individual assets of a business has anything in common with the idea of income contained in their second definition, which I should have thought referred to considerations of *aggregate* capital value, which is different from, and has no close correlation with, the sum of individual asset values. Lord Keynes, in the *General Theory* defines " income " in general terms as sales less expenditure plus (or minus) the increase (or decrease) in the value of equipment.[2] Apparently he does not mean by this that fluctuations in the value of the whole business, that is, including " goodwill ", should be brought to account. Nevertheless, he apparently regards asset value appreciation as income. The accountant does not. The basis of the accounts of a business undertaking is the allocation of costs and revenues. Balance Sheets and Profit and Loss Accounts do not record values. Whilst figures of assets are ascertained and put in the Balance Sheet each year, it should be appreciated that these figures are costs incurred and not values. It is a little difficult to make this sufficiently convincing because there is, I think, a certain amount of confusion in the accountancy profession in relation to the nature of figures of assets. Many accounts show stock-in-trade " as *valued* by the officials of the company ", and there is the well-known rule of stock " *valuation* " at cost or market *value*, whichever is the lower, which appears to imply that profit figures take account of depreciation in capital-value. It may seem at first to be a somewhat nice point of metaphysics, but it is my contention that the use of this rule is not a method of " valuation " properly speaking, but of allocating expenditures against the revenues to which they relate.

However, I will not make this into a detailed exploration of accounting theory. The broad principle which is followed in ascertaining profits, and by which existing procedure stands or falls, is not in serious question amongst accountants; it is that profit is the surplus of revenue earned from the supply of goods or services over the cost of supplying them and the cost of promoting their sale. This is quite different from the idea of profit as being accretion in capital value, which idea Fisher charges us as entertaining and using, and which some economists think of as the correct mode of considering profit. A business which is earning a diminuendo current surplus will probably have a falling capital value, and the proprietor may be " losing " (in a total sense) over a series of years. There will nevertheless be a profit in each year's accounts, *i.e.*, the current surplus, and whilst there will be a greater " capital loss ", the accounts will not, generally speaking, show anything of this. Nor, in my opinion, should they do so.

The most useful approach to this problem of the nature of profit is to take a simple case in which complications such as holdings of trading stocks and the use of durable equipment are eliminated. Assuming year-end stocks as nil, we may take a business which has sales less purchases (there being no other working expenses) of £10,000 and £8,000 in 1943 and 1944, respectively. Let us say that the business was purchased on January 1st, 1943, for £100,000 with an expected return *in perpetuo* of 10 per cent. per annum. We may further postulate that on January 1st, 1944, a fall in net receipts to £8,000 per annum was foreseen, but that the expected rate of interest appropriate to a risk of this class remained throughout at 10 per cent. per annum. In 1943 there was an accounting profit of £10,000 and a capital loss of £20,000, making the proprietor £10,000 worse off in the year. I leave to the economist the precise significance of this net worsening of £10,000. I feel confident myself that the profit figures are £10,000 and £8,000, and that those yields are the figures which the proprietor should regard as his income. He may, of course, like to take into account his diminished prospects of income—he may even like to think of this as a capital loss of £20,000; but I think he would be right in viewing this " windfall loss " (as Lord Keynes would describe it) in a different light from his current position on trading account. There is a diminution in yield, *and*, as a consequence of this adverse trend, there is a fall in the capitalised value of the yield; but these are separate factors so far as the owner's personal economy is concerned. The accountant's division of these factors into the compartments " income " and " capital profit or loss " seems to me to be in accord with a common-sense outlook, and since it is a division which is in fact made by the entrepreneur in our present economy, it is a distinction which must have some recognition in economic theory.

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[1] *Economic Journal*, June–September, 1943, p. 264.

[2] His distinction between " income " and " net income " and the significance of " supplementary costs " are unimportant in this context.

It is quite easy to speak glibly of a fall in value from £100,000 to £80,000, in relation to current earnings of £10,000, but there is the further point that whilst the current earnings of a business can be ascertained with reasonable accuracy, the same cannot be said of the value of the undertaking. It is sometimes thought that this lack of verifiable evidence for fluctuations in capital values is the reason why accountants do not make allowance for them in annual accounts. Whilst it is certainly true that accountants do ignore some things because of their incommensurability, this is not the reason for ignoring these fluctuations. Even if the appropriate figures could be ascertained, current yield would still be the correct profit figure in the eyes of the accountant.

Before the reader passes judgment on this view of the nature of profit, I must invite him to consider a distinction which I feel might be made between business profit and personal income in the case of holdings of shares in public companies. On this point I am not on such safe ground, for I doubt if it would be possible to persuade accountants in general to commit themselves to an idea of what is a person's income from his shareholdings, particularly where there are purchase and sale transactions involved. I doubt if many would consider the shareholder's income to be his apportioned share of profits during the period in which he held shares in a company. The idea of cash dividends as a measure of income would be more popular; but there are some obvious difficulties. For one thing, the investor may pay close attention to cum.-div. and ex-div. margins and take advantage of small price differentials; he may deliberately sell shares " full " of dividend and buy newly ex securities simply from a tax advantage standpoint. If there can exist these deliberate interchanges between dividend yield and dealing margins, one may well ask whether any distinction exists between current yield and capital accretion. There are many clear instances where such a distinction would seem absurd; thus, a redeemable debenture stock may be purchased above par when the inescapable " loss " on redemption is sufficiently offset by a high interest rate during the period of its currency. The proper treatment in this case would be to amortise the premium paid over the life of the stock. It seems, therefore, that in the case of the " absentee-investor ", the notion of current yield lacks precision, and, if it means anything at all, it certainly does not mean share of profit as ascertained by business Profit and Loss Accounts. I should be disposed to suggest that income on such investments means total cash yield in dividends and interest plus " capital profits " (less losses). Obviously " capital profits " includes " unrealised " surpluses, for where there is a ready market for a security, the surplus on a block still held has the same significance as that on one which happens to have been turned into cash. There remains the important qualification that for some purposes it is more important to know what people regard as their incomes than what " really " are their incomes, and it must be admitted that some people do in fact make some sort of a distinction between capital and income in relation to investment holdings. They regard a surplus on sale in a different light from a cash dividend. It is outside the scope of my study to pursue this subject to any kind of precise conclusion as to the nature of personal income, but if one is to have a clear view of the nature of profit on business operations, it is necessary to have a general idea of the relationship of profit to income. Perhaps it is sufficient to appreciate that profit is not necessarily a mere component of income, that what is " capital " from a profits point of view may be " revenue " from an income standpoint.

Another illustration of this distinction between the notions of the net revenue of a business undertaking and the revenue (or income) of the proprietors of the concern may be given. This relates to profits which were earned prior to the purchase of shares, and which are applied to the payment of dividends after such purchase. This problem arises in relation to holding companies' accounts, where good accounting practice favours the allocation of such encashment of a subsidiary's pre-acquisition profits to capital—against the cost price of the shareholding. In this case, profits are sometimes brought to account as they are earned by the subsidiary without waiting for payment of dividends (which payment, of course, is merely a domestic financial matter in the case of wholly-owned companies). There exists, therefore, a difference in the calculation of revenue from an investment as compared with the position of an individual person acquiring a block of shares on the market. He will be most unlikely to make any such calculations of profits accumulated prior to purchase. The distinction is between investment which carries control and is intended as a more or less permanent commitment, and investment which is readily reversible on the slightest hint of a market " over-valuation " (and in passing it may be observed that in the former case no reliable capital value is ascertainable, whilst in the latter it is).

We may pass from this to a more complex case of holding company adjustments, where at the time of acquisition of a controlling block of shares in another company, there is a valuation made of the subsidiary's assets. The amount paid for the shares may be partly based on these revised values of the assets, and estimates of future earnings may take account of amortisation of equipment computed on these figures. Generally speaking, no adjustments will be made in the books of the subsidiary company in relation to any such revaluations; indeed, if any minority interests remain, it would be improper to revise its accounts in this way. Nevertheless, the book figures of the subsidiary's assets will have no significance from the holding company's standpoint, and the true revenue of that company from its investment should take account of any difference between the amortisation charged in the subsidiary's accounts and that based on the revaluation, and also of any other factors, such as revision of reserve and provision balances, to which weight was attached at the time of purchase. Some attempt at effecting these rather involved adjustments is sometimes made, and it illustrates my point that the problem of revenue is bound up with the domestic circumstances of the beneficiary—person or corporation. Whilst the accounts of a concern are prepared on an assumption of continuity of the entity, no other basis being possible in many cases, the accounts, or other calculations of revenue, of the beneficiaries take into consideration factors outside the revenue-earning business's purview, with the result that personal incomes are not necessarily, nor even probably, equal to business profits in the aggregate.

Now that I have stated a case for the interpretation of *profit* as meaning *current yield*, fluctuations in capital value being ignored, not because of the difficulty in assessing them but because of their irrelevance, I may turn to the slightly more difficult case of a business which carries a stock of commodities over its Balance Sheet date. This is not easy, for I cannot disguise the fact that there is a good deal of difference of opinion in the accountancy profession in relation to the ascertainment of Balance Sheet figures for stock-in-trade. There is even more confusion as to the precise philosophy which lies behind the various methods of fixing these figures. This is very unfortunate because to economists an exposition of the system of philosophy might be of more use and interest than particulars of the accounting procedures found in use.

The principle upon which accounts should be prepared (and largely *are* prepared) is that asset figures are amounts of expenditure excluded from charge to current revenue as relative to subsequent earnings. Assets are not taken at their " values " as that word is generally understood. The accountant often speaks of " valuation " when he does not mean by it what other people mean when they use the word. He generally uses it in a specialised conventional sense, in which I think it would be preferable to speak of " assessing " a figure rather than " valuing ". My own view is, that the stock of a business should be assessed at the amount expended in acquiring and (if it is a manufacturing concern) in processing the marginal materials equivalent to the stock actually held at the end of the accounting period. These are not necessarily the same physically as the last items of acquisition, since, within wide limits, it is purely a fortuitous matter which particular items of stock of similar types are taken from those on hand to meet customers' orders. The significance of this " first in–first out " method is that it abstracts from the expenditure to be charged against revenue from sales exactly that figure of cost which, if it had not been incurred, i.e., if the stock had been allowed to run down to nothing, would have left undiminished the capacity to reach that volume of sales.[1] This method of computing profits requires that we " value " stocks on hand only in the sense of ascertaining their cost

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[1] The other important view of stock " valuation " is that it should be based on original cost of similar stock items. This is approximately the same as saying that revenue should bear, not the cost of the items actually sold, but the current cost of replacing them. This point is made by Mr. K. Lacey in ECONOMICA, February, 1944, " Commodity Stocks and the Trade Cycle," and considered further by him in the issue of February, 1945, under the title, " Some Implications of the First In–First Out Method of Stock Valuation."

(on a first in–first out basis). Still lingering in the general notion of profit is the idea of accretion in value ascertained in the " single-entry " manner of valuing the assets of a business at two dates and adding withdrawals by the proprietor. Even the common form of Trading Account which begins with opening stock on the debit and finishes with closing stock on the credit, added to sales, fails to make it clear that this stock adjustment is merely an allocation of production costs to the periods in which the goods are sold. I consider that there are some defects in the way in which accountants view this question of stocks and the incidence of market value, and that in consequence the accounting treatment of this problem calls for some improvement. In particular, the practice of adding overhead expenditure in the prices at which stocks of finished goods are taken in a manufacturer's accounts, runs contrary to the notion of the stock adjustment as being simply an elimination of the marginal cost of the unsold goods on hand. During periods of stock accumulation, profit is incorrectly inflated by an increased amount of oncost included in the asset figure, but not related to any increase in rate of overhead expenses. In fact, such stock accumulation may be a result of an unexpected decline in sales, the falling surplus on which will be partly hidden by this erroneous credit for increased oncost in the stock figure.

That particular question is perhaps one which accountants ought to be able to decide for themselves, but as it is closely related to this marginal-cost-of-unsold-stocks adjustment it is worth this brief mention. The famous " lower-of-cost-or-market-value " rule of stock " valuation " is more difficult to dismiss briefly, and is interesting as illustrating the philosophy behind the accounting measurement of profit. Some accountants have said that this rule is illogical and inconsistent with the accounting idea of profit. I think the rule is an excellent example of the way in which common sense is ahead of pure theory in the early stages of the development of a science. Now that a structure of theory is being built up, but is still somewhat inadequate, it becomes curiously difficult to find the theory that fits in with what seems *prima facie* to be correct procedure. In the case of a retailer of ladies' gowns, the purchase of a season's gowns will be expected to show a gross surplus in the aggregate, but it is quite likely that some of the gowns will not realise their specific cost price. " Fashion " goods are sold partly at a high profit and partly at practically no margin, or even at a loss, and the purchase of the unsuccessful lines is not normally a matter of bad judgment but simply a consequence of its being impossible to foretell just what direction the largely fortuitous moves of Fashion will take. The trader here does not regard the specific price of one gown as necessarily specifically attachable to that gown. In other words, he will recover part of the cost of the unsuccessful gowns in selling the successful ones. There is a transfer of part of the costs of the bad " buys " to that of the good " buys " corresponding to the reckoning of physical spoilage of materials in manufacture as part of the cost of the good output.

From this it follows that in taking the unsuccessful lines into stock at something less than their specific purchase prices (a process which, though it is rather a rough guess, is an application of the lower-of-cost-or-market-value rule) the trader is still assessing a figure of cost to be carried forward. Market values *as such* are not brought into accounting statements : they are used as a factor of reference in apportioning costs to the correct periods. This is not just a matter of words. The idea that values are brought to account (as perhaps they were in the days of " single account " book-keeping) still clogs the proper development of accounting theory and forms an obstacle to the appreciation of the nature of the accounting process on the part of shareholders, economists, and laymen generally.

That is the general philosophy which justifies the consideration of market values of stocks. I think I have said enough for the economist to be clear enough now to pass judgment on the principles of stock " valuation ". There are details in the application of these principles which are important, such as the use of average costs in some cases, and the difficulties involved in a thorough-going application of the rules where there is a very large volume of business and a complex system of stores and cost records involving transfers of products at various stages of manufacture. However, these remain merely details, and there is not enough agreement about broad principles for the details to be worth discussing. I am most anxious that we accountants should have the benefit of the views of economists on these questions concerning the nature of profit. The more interesting problems are those which relate to the disparity between aggregate accounting profits and " real " incomes. To most accountants, it does not seem important to consider the maintenance of a constant physical stock in ascertaining profit. If a business has ten tons of *X* which cost

£100 and which it sells for £120 and then buys another ten tons for £110, it would seem to the accountant that there is a profit of £20. If the proprietor had decided to withdraw this amount and buy only nine tons and two hundredweights of *X* (costing £100 and probably selling for £120, as did the ten tons), he would indeed have been reducing the physical scale of his activity, and quite possibly be enjoying a reduced " real " income. But although £20 may have a diminishing purchasing-power, it is nevertheless the true profit on trading, and though business men will thereby enjoy enhanced profits during a general advance in prices, they are quite genuine profits. Probably they will see to it that they increase their stocks during this period of advance, and will curtail them drastically during a decline. If such a notion as a " normal " or " base " stock is tenable at all, it would seem to be such an imprecise factor as not to be susceptible to accounting recognition. It seems as far removed from the ordinary process of accounting for material costs as is the attempt at quantitative expression of price-level movements from the measurement of aggregates of national income and outlay. As for taking account of the maintenance of the physical scale of output (*i.e.*, by financial provision for replacement of equipment used or materials sold), it would seem that the only plausible argument—that we must make provision for the maintenance of the proprietor's level of " real " income—leads us to a difficult position:—

(*a*) for one thing the rise in the cost of living probably will not be equivalent to the rise in the prices of the materials which a particular firm handles, so that an adjustment by reference to cost of living indices rather than to the variation in the prices of the materials actually dealt in would seem more nearly logical;

(*b*) secondly, the thorough going application of this would lead us to Professor Hicks' Income Number Three, in which the equation of current and expected " real " income is to be achieved.

The task of translating either of these ideas into practice would seem to me to be a distasteful one. I am, however, open to the suggestion that accountants should think about this problem. Business managements may wish to save out of profits to expand in a financial sense sufficiently to maintain a constant physical scale of activity during a period of rise in prices, and there is a case for reserves being made to meet such rises. My own judgment of this is that any such calculations lie outside the field of ordinary profit measurement; it is not merely that they are less precise, they would appear to belong to a different order of measurement. The appearance of precision possessed by some of the methods which have as their basis this idea of provision for maintenance of a " real " position is a snare and a delusion. One wonders how the " Lifo " method would achieve the desired results when a business changed its class of materials handled so that the stores records had to take account of current prices. Even more difficult would be the case of engineering construction concerns whose successive ventures are more or less unique, where it is hard to see how any other basis than actual cost could be applied. One might, of course, employ index numbers of the price level to deflate the stock figures as normally computed, but even if one were to accept this idea of providing for the maintenance of the physical amount of stock, the use of index numbers in making the calculations leads one onto somewhat shifting ground.

The increased popularity of " Lifo " and other similar methods of stock " valuation " is apparently a consequence of the idea that there is something " different " about the benefit which a holder of stocks secures during a period of rising prices. Perhaps, indeed, there is something different about boom profits, namely, that they are not a good guide to the long-term earning capacity of a business. But nevertheless, the advantage of holding stocks at such a time *is* a very real one. The entrepreneur will usually do his best to acquire considerable stocks at such times, and to reduce them if a slump seems likely, when the margin between cost and selling price may be eliminated if there is appreciable lag between expenditure and revenue from sale. The argument gains some weight from taxation considerations, and from the notion of " normal stock ", a notion on which I have ventured to express scepticism. Let us suppose that firms *A* and *B* carry on similar classes of business, and each has available funds of £10,000 on the 1st of January, on which date the management of each firm is wondering whether to buy 1,000 tons of commodity *Z* at the current price of £10 per ton. Let *A* decide to make this purchase and *B* to keep the money in the bank, *A* selling 1,000 tons during the year, at an average price of £15 per ton, and buying a further 1,000 tons, at £14 per ton. Now in relation to these transactions, *A* has £1,000 in the bank and possesses 1,000 tons of *Z*, whilst firm *B* has still £10,000 in the bank. If the correct stock price for *Z* were only £10 per ton, the original price, *A*'s profit would be only £1,000. But *A* is much better off than *B* than by a mere £1,000. At £14 per ton (and the 31st December price may exceed this) *B* can buy only 714 tons with its amount of cash of £10,000. If it did make this purchase, *A* would have £1,000 more cash and 286 more tons of *Z* than *B*. At £14 per ton, this additional stock holding would have a Balance Sheet " value " of £4,000, which, with the additional cash, makes up the £5,000 which I consider to be the correct figure of profit on these transactions, being the difference between the revenue of £15,000 and the cost of the goods sold—£10,000. There is a perfectly valid sense in which *B* may be said to have lost by holding £10,000 cash throughout a year of rising prices, simply because the cash will not buy so much at the end of the year as it would at the beginning. But to take account of a " loss " on cash held is to depart from the basic notion of accounting for profits as the measurement of a money surplus irrespective of considerations of the " worth " of money. Any such calculations in terms of purchasing power and " real " income seem to me to lie outside the scope of profit measurement. At the most, as I have indicated, the accountant might give some consideration to these factors in dealing with the reserve and dividend policy, that is, with the savings to be made out of profits to finance additional investment in the business.

At many phases of the accounting process we have to deal with approximate figures rather than absolute ones. Stock " valuation ", whatever the method employed, is just one feature which involves not only calculation but intelligent guessing. But there are some things which are incommensurables, either inescapably or at the existing stage of development of accounting technique and of business economics. The allocation of advertising expenditure against resultant revenue is one of these incommensurables. It is, I think, of the latter class, and it is my hope that one day accountants may be able to make calculations of such allocations with sufficient confidence in them to employ the results in business accounts. At present, the study of the degree of lag between publicity and its results has received inadequate attention from economists and accountants, and such figures as are sometimes carried forward as assets, " Deferred Advertising ", or the like, are not the result of systematic calculation. In my view, such figures should not be used at all in the existing state of knowledge of the connexion between costs and revenue, but the fact that they do sometimes appear is an indication of the shape of things to come.[1] In the meantime, the best the accountant can do is to charge advertising expenditure against revenue as it is incurred and to show the amount expended in the final summary of profit. I am not concerned with the question of disclosure in published accounts, that is outside my province. I mean simply that a Profit and Loss Account does not fulfil its description unless—if I may be allowed a semi-paradox—it gives amounts of these incommensurables (*i.e.*, not measurable in relation to appropriate period of charge), so that the reader may weigh them in his mind as giving rise to a degree of variability in the measurement of the final profit. Provisions for contingencies fall into this class of profit deduction, and very commonly a contingency undoubtedly exists, but is so uncertain in amount or in degree of probability of occurrence, as to make it impossible to regard it as adequate simply to hide the provision as an accrued expense along with ordinary costs incurred.[2] Where such factors intrude on the validity of the final profit figure, the accountant should still be as careful as is possible about the commensurable portion of that which he is subjecting to classification and measurement. All transactions susceptible to full accounting treatment should be so treated, and the result stated along with the intransigeant factors so as to show a net figure which is the periodical " profit " to be viewed as subject to obvious limitations to its utility as a guide to future action.

It may be seen from this outline survey that, unlike the economist, who can apparently reject " income " as a tool of economic analysis, the accountant must rush in where Professor Hicks fears to tread. We accountants grant the attribute of objectivity to " profit " if not to " income ", and attempt to apply a measuring-rod to it however clumsy we may have to be about it. " The purpose of income calculations in practical affairs is to give people an indication of the amount which they can consume without impoverishing themselves ".[1] This is a proposition to which we can all subscribe. It is in the deductions made from this proposition that there exists a difference between the views of economists on the one hand and of accountants on the other, a difference which is worth exploring, for even if " income " is not a " logical category ", it—or at least " profit "—is something which is put into figures by accountants, and for that reason at least attains economic significance as a datum in entrepreneurial and domestic decisions. I cannot therefore see that economists could get along without using our ideas of " profit " even if we were quite wrong. But in the interests of society as well as in those of consistency in our respective sciences, I think it is time we co-operated a little more to discover whether we accountants are right; and if not, to decide what should be done about it. Another accountant, Mr. F. Sewell Bray, has said:—

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[1] Cf. Neil H. Borden, *The Economic Effects of Advertising*, which is a study (*inter alia*) of the effect of advertising on sales.

[2] I am afraid that provision for the contingency of future trading losses is sometimes made in this way in published accounts. The justification for making any such charge against revenue is not particularly good: there is no justification whatever for concealing the fact that it has been done—but I digress.

> " Accurate accounts are part of the apparatus which economists must summon to their aid in the quest for truth and *ultimately* the quest for the good in the field of both social and economic enquiry."[2]

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[1] *Value and Capital*, p. 172.

[2] Under the title " The Philosophy of Accounts," in *The Accountant*, August 12th, 1944.

Notes on N. W. Senior's *Political Economy*

By **John Stuart Mill**

[The following notes are reproduced from an interleaved copy of the first (quarto) edition of N. W. Senior's *Outline of the Science of Political Economy* (reprinted in 1836 for private circulation from the *Encyclopædia Metropolitana*) which is now in the possession of Senior's grand-daughter, Mrs. A. St. Loe Strachey. The copy bears on the title page, in ink, N. W. Senior's signature and the remark " (autograph notes by J. S. Mill) ". The notes, with one exception noted below, are written in ink on the white sheets bound in between the pages and are unmistakably in J. S. Mill's hand. The precise point in the text to which they refer is indicated by an asterisk. There are also a few further notes pencilled on the margins, but these have not been reproduced as they are of no great interest and because it is not certain that they are also by Mill. Mill's spelling and punctuation have been followed as closely as possible, though, as is usually the case with his manuscripts, it is often impossible to decide which punctuation sign has been intended.

The references before each note refer I to the quarto edition from which the notes have been reproduced (the pagination of which commences with p. 129) and II to the corresponding passages in the reprint from the second (octavo) edition of the *Encyclopædia Metropolitana*, 1850 and later, and to the edition in the " Library of Economics " (edited by Dr. P. N. Rosenstein-Rodan and published by George Allen & Unwin, London, 1938), which is a photographic reproduction of the second edition.

Thanks are due to Mrs. A. St. Loe Strachey for permission to reprint these notes.

F. A. H.]

Ad I, p. 134, end of page,
II, p. 15, beginning of page,
where Senior quotes James Mill's and Malthus' definition of " demand ".

It seems to me necessary, when we mean to speak of the *ratio* between the demand for a commodity & the supply of it, that the two quantities should be, in the mathematical sense, homogeneous—that both of them should be estimated in numbers of the same unit. Now as ordinary usage makes it almost impossible to use the word *supply* in any sense but that of *quantity of the commodity*, viz., the quantity actually in the market or ready to be brought into it, *demand* must I think be used also in the sense of quantity of the commodity, & in that sense it can only be taken to mean, the quantity for which at the market price, purchasers can be found. These definitions being adopted, the fundamental theorem of value must be expressed thus: The value of a commodity in any market will always be such that the demand shall be exactly equal to the supply.

Ad I, p. 135, beginning of page,
II, p. 15, middle of page,
to the sentence : " The deficiency of wheat would not give to the consumers of oats and barley any increased power of purchasing them, nor would the quantity purchased or consumed be increased ".

But the quantity for which there would be purchasers at the previous market price would be increased, therefore there would be increase of demand in the sense I propose giving the term. The demand, in my sense, having thus become greater than the supply, the value must rise until the rise either calls forth an addition to the supply, or brings back the demand to what it was before, & in either case restores the equality of supply & demand.

Ad I, p. 138, end of page,
II, p. 24, second paragraph,
to the phrase : " as limitation of supply is essential to the value of labour itself ".

I do not think limitation of supply essential to the value of labour. Labour being painful, would not be incurred without some sort of equivalent pleasure and advantage even if labourers could be multiplied indefinitely by a volition, or if every man could work a hundred thousand hours in the four-and-twenty. Unless indeed the labourers when created could be constrained to work by a physical force equally unlimited in supply & which did not act through their own will, but employed their muscles as mere machinery. For even to forbear stripes is *some* equivalent for labour.

Ad I, p. 146, end of first paragraph,
II, p. 42, second line,
to the sentence: "after once a colony has been planted, its principal increase arises, not from the comparatively scanty recruits whom it receives from home, but from the unrepressed force of human fecundity."

These statements have been true hitherto, but only because the emigration which took place has not been made available to facilitate further emigration. This could only be done by national arrangements, & emigration has never till now been a national concern.

Ad I, p. 148, middle of third paragraph,
II, p. 46, last paragraph,
to the sentence: Mr. Malthus's "admission that, 'in the progress of society, the probability is that the evils occasioned by the pressure of population against food will be mitigated', is opposed to Mr. M'Culloch's statement".

I think Mr. M'Culloch's expressions only mean (considered with reference to the general scope of his argument) that the power of increase *when fully called into action* would in the long run be an overmatch for any increase in the means of subsistence.

My father's expressions seem to me equally reconcilable with the true view. The condition of the population, being one of poverty, proves that population has borne too great a ratio to capital, through the effect of a "tendency" in Whately's *first* sense, which though partly impeded has not been sufficiently so.

No political economist, except Malthus in his first edition, seems to me to be chargeable with more than having used expressions which might be, & which occasionally have been, misunderstood, and it seems to me to throw very unnecessarily a shade of apparent doubtfulness over the true view of the subject & over the truths of the science in general, to represent the great authorities on the science as having been at war on so fundamental a point, when I do not believe there was an atom of difference in their real opinions.

[The concluding paragraphs of the section in a great measure obviate the necessity of this remark.]

Ad I, p. 153, end of first column,
II, p. 59, italics in last paragraph,
to the sentence: "*By the word abstinence, we wish to express that agent*".

I question if abstinence can be called an agent or an instrument of production. Could not you call it a *condition*? And might not the word *saving* be used, not to supersede but occasionally to alternate with the term abstinence? Labour, natural agents, & *saving*. Besides, in order to employ productively what serves to feed me, I do not *abstain* from the enjoyment of it, I merely labour *while* I consume it.

Ad I, p. 154, middle of first column,
II, p. 61, middle of long central paragraph,
to the sentence: "the only difference being, first, that his knowledge remains unimpaired until either his death or his retirement from business makes it suddenly valueless".

Can it be said to have become valueless merely because he ceases to use it? when if he chose to bring it into market he could obtain a value for it?

Ad I, p. 155, end of second paragraph,
II, p. 63, end of second paragraph,
to the sentence: "On the other hand, the materials and stock-in-trade of an iron founder would be circulating capital according to Smith, and fixed capital according to Ricardo".

I think Ricardo & my father habitually gave the name fixed capital to all those portions of capital which are not consumed & reproduced within the year—& that they included in each class the same objects as Adam Smith, though their definition may have been different.

Ad I, p. 160, two-thirds down in last paragraph of first column,
II, p. 75, last line,
to the sentence: "The mass of the inhabitants of Egypt, Persia, and Burmah, or to go as low as perhaps it is possible, the subjects of the Kings of Dahomi and Ashantee, enjoy security, if we compare their situation with the ungoverned inhabitants of New Zealand".

If so, it must be from some peculiarity in the case of the New Zealanders. An oppressive government which is a vigorous one, is no doubt better than an anarchy, but most bad governments include all the evils of anarchy, for they plunder the people and leave them to protect themselves. We see that in the bad governments of India.

Ad I, p. 160, middle of first paragraph of second column,
II, p. 76, end of first paragraph,
to the sentence: "In representative governments they arise principally from their unskilfulness".

A good deal from their bad intentions too, I think, witness the Corn laws—slavery—and many other evils.

Ad I, p. 161, towards end of second paragraph of second column,
II, p. 79, towards end of first paragraph,
to the sentence: "It is probable, however, that Adam Smith's real meaning was, not that the identical supplies which will be wanted in a course of progressive industry must be already collected when the process which they are to assist or remunerate is about to be begun, but that a fund or source must then exist from which they may be drawn as they are required . . . The painter must have his canvas, . . ."

Not only that; but there must be an equivalent fund stored up in the possession of the painter himself, to enable him from it & its intermediate profits to supply himself with necessaries during the whole time; unless he be a mere workman receiving wages from an employer.

Ad I, p. 162, end of long paragraph in first column,
II, p. 80, end of first paragraph,
at closing bracket.

It might be well to remark that the portion of the price of any article which goes to remunerate labour and abstinence long past, becomes infinitesimal when we go a very few stages back. The *recent* labour & abstinence carries off all but an inappreciable fractional quantity—& therefore all which is capable of operating as a motive.

Ad I, p. 186, end of second paragraph in second column,
II, p. 95, end of fourth paragraph,
to the sentence: "But it appears to us to be the most philosophical arrangement to consider it [the portion of the annual produce which is employed in the production or the support of brute or inanimate capital] as rent, wages, or profit, according to the character of its proprietor, without regard to its subsequent destination".

I cannot see the propriety of calling that which merely repays the capitalist for his advances, rent, wages, or profit. As much of it indeed as he destines to pay his next year's rent or wages, may be termed rent or wages because that is its actual application: but I do not see how what goes to keep up his machinery or stock can be called profit, that is, remuneration for abstinence, since he would have had it and could have consumed it if he had not abstained. He had it when he began business. It may have been profit once, or wages, or rent: but it has long ceased to be so.

Ad I, p. 170, towards end of third paragraph in second column,
II, p. 100, towards end of third paragraph,
to the sentence: "When he termed profit a part of the cost of production, he appears to us to have meant not profit, but that conduct which is repaid by profit".

I conceive rather that he & every one else, when they included profit in cost of production, meant not the profit of the actual producer, but the profit of the producers of his machinery, & other articles of capital which formed part of the price paid, & thence part of his own outlay, on which his profit is to be estimated. I am confirmed in this opinion by remembering that it is chiefly that previous profit which affects price: for it is chiefly in that way (though not wholly so) that profit enters more largely into the conditions of production of one commodity than into those of another.

Ad I, p. 173, middle of fourth paragraph of second column,
II, p. 107, middle of last paragraph,
to the sentence: "The price falls in consequence of the increased supply".

It does not seem to be necessary that the price when it begins to rise should overshoot the mark. Probably all that happens is a gradual rise of the price of the raw produce, & (proceeding *pari passu* with it) a gradual extension of cultivation a *little* further off.

Ad I, p. 186, first sentence of second paragraph, second column,
II, p. 138, first sentence of first new paragraph:
"Mr. Ricardo seem to have forgotten that the quality which enables land to afford rent, namely, the power of producing the subsistence of more persons than are required for its cultivation, is an advantage without which rent could not have existed".

But manufacturers possess the same advantage without the accompanying disadvantage. They produce the clothing, &c., of more persons than are required for carrying them on. All industry has this quality; & it is the disadvantage only which is peculiar to land, & which causes land to pay a rent & machinery not. This is all that Ricardo said; & on the whole I do not see any value in this criticism upon him.

Ad I, p. 189, end of first paragraph of first column,
II, p. 143, end of paragraph,
to the sentences: "Now we know that general variations in the amount of wages to the amount of one-tenth or one-fifth, or to a greater extent, are not of unfrequent occurrence. Yet who ever heard of their producing such an effect on profits?" [namely, "utterly to destroy all profit whatever".]

It would if the capitalist were only receiving profit on what his *labourers* produce. But he is receiving it on his fixed capital too.

Ad I, p. 189, first sentence of third paragraph, first column,
II, p. 144, first sentence of second paragraph:
"Did Mr. Place ever apply this doctrine when his men asked for higher wages on a general mourning?"

That is not a *general* rise of wages, & therefore can be compensated by a rise of price. The objection seems to me irrelevant.

Ad I, p. 190, in first column, last paragraph,
II, p. 147, end of fifth paragraph,
to the sentence: "The whole of the confusion has been occasioned by the verbal ambiguity which we have pointed out, and would not have arisen if Mr. Ricardo had used any other adjectives than *high* and *low* to express a larger or smaller proportion".

It seems to me that the confusion stated to have been occasioned is over estimated. I do not believe that anybody was thinking of low wages in France in reference to the comforts of the labourer, but in reference to the cost of his labour to the employer—which is the only sense in which wages either affect profits or enable a producer to undersell—& the question was, which of the two effects would take place. The cost of wages to the capitalist is a phrase synonymous with proportional wages, or wages in Ricardo's sense.

Ad I, p. 193, first third of last paragraph of first column,
II, p. 154, first paragraph, at second semi-colon,
to the sentence: "We know of no definition of that term [capital] which will not include many things that are not used by the labouring classes".

The use of the word Capital requires to be very deliberately reconsidered by pol. economists, but I think the word should be used in a sense which would cancel this paragraph. I would call capital, what anyone possesses which he designs for productive use either in itself or in what it will purchase. If all the food in this country were to be destroyed to-night, & every capitalist determined still to keep up the extent of his productive operations, I should say our capital was undiminished, though much of it must change its shape before it could be used as capital. If half the plate-glass was destroyed & and the remainder sold for as much as the whole did before, I should say the capital of the country was *not* diminished: the whole loss would fall on the revenue of the purchasers, & next year's productive operations would be unimpaired. I think a peculiar use of the word capital confuses exceedingly the latter part of this Treatise.

Ad I, p. 194, first sentence of second paragraph, second column,
II, p. 156, last paragraph,
to the sentence : " This reasoning assumes that the landlord, while resident in Ireland, himself personally devours all the cattle produced on his estates " ;

No. It only assumes that whatever he did not personally devour would continue to be exported notwithstanding his residence in Ireland. And it surely would; since it was exported before not for his own eating but for that of foreigners.

This seems to me to destroy the distinction here set up between countries which do & which do not export raw produce.

What the landlord paid out of his income, directly in wages of labour, would certainly benefit the native labourers whether the country exported raw produce or not. But to the extent of the food wanted by the additional native labourers whose labour he so purchased, *more* raw produce must be *grown*.

I think confusion worse confounded is the result of calling nothing capital till it is in the state in which it can be used as such. According to my view, it is net increase of food which raises wages: it is certain purposes in the minds of capitalists which make them bid higher for labour, *that* raises wages, & the rise of wages *causes* the production or importation of the necessary increase of food.

This is the only difference of any importance between the doctrines of the science as I conceive them and those taught in this book.

Ad I, p. 197, penultimate sentence of first paragraph, second column,
II, p. 164, penultimate sentence of first paragraph,
to the sentence : " The number of books extant at any one period before the invention of the printing-press was probably smaller than that which is now produced in a single day ".

These instances are not conclusive, for in order to produce this increased quantity, capital may have been drawn from other employments, & the whole annual produce of the country diminished, though the production of that one article has increased.

Ad I, p. 201, penultimate sentence of first paragraph,
II, p. 172, penultimate sentence of first paragraph,
to the sentence : No one would maintain . . . that if it were the practice, as it is in semi-barbarous Countries, to maintain servants to produce within their masters' walls the commodities which we are accustomed to purchase from shops, such as the fine clothes or furniture to which Mr. Ricardo alludes, the demand for labour would be increased ".

Surely the demand for labour *would* be increased: the commodity would be produced by the capital of the purchaser. It is the same thing as if the purchaser saved his income to set up a manufactory. Surely if that did not increase the demand for labour, nothing would.

Ricardo seems to me quite right. The doctrine here maintained in opposition to him, arises I think from what I have ventured to call a misuse of the word capital.

If I ceased to buy (suppose) silks, and with the money they cost, hired servants, I should add to the fund for the purchase of labour. The silk manufacturer's capital remains as before; & I come into the market too, as a competitor with him. The commodities on which the labourers will chuse to spend their increased wages, are not yet increased, it is true; but (I contend) that increase always *follows* instead of preceding & causing the rise of wages. Rise of wages means, the labourer's having more given him—more commodities, more exchangeable value, for his labour: his tastes & wants & the demand they generate, *afterwards*, determine what commodities shall be produced to meet that demand. Thus the series of facts is not, 1st, increase of necessaries & labourer's luxuries 2d, consequent rise of wages. It is, 1st, increase of commodities of any kind destined by their owners for productive employment, 2d, rise of wages, 3d, increased demand for necessaries & labourer's luxuries; 4th, increased production of them.

Ad I, p. 201, third paragraph of second column,
II, p. 174, first paragraph,
at the margin against the underlined words : " to the use of the labouring population ".

I would say " to be expended in the purchase of labour ".

If my view is correct all the sequel of the book is, of course, unnecessarily complex & obscure, as well as occasionally erroneous.

Ad I, p. 204, end of penultimate paragraph,
II, p. 181, end of second paragraph,
to the sentence: "They draw their subsistence not from the common fund, such as it otherwise would be, but from the addition made to that fund by that extraordinary productiveness".

This would be true if the landlords took the men into their houses & maintained them while they produced those articles. But the fact is that these labourers would not exist unless there was a capitalist who chose to devote a portion of what he might enjoy, to the purpose of paying labourers to manufacture goods for the use of landlords. Now if rent were all thrown into the sea & this capital remained, the labourers would still receive value from the capitalist which they would want to lay out in food & (unless the power of all soils were already tasked to the utmost) could get it. I contend therefore that they *are* maintained out of a common fund & not out of rent.

Trinity College, Dublin, and the Theory of Value, 1832-1863

By R. D. Black

The history of economic doctrine is a subject out of which conventional general ideas are very apt to arise. It is a terrain with many varied and complex features, but the majority of those who survey it do so only broadly and recall afterwards merely the general topography and the chief landmarks.

One very prevalent idea resulting from this process is that the period before 1870, in English economic writing at least, marks the dominance of labour or cost-of-production theories of value, whilst the period after that date saw the ascendancy of the " subjective " or " utility " approach to value questions. Of late years, however, this conception has undergone some revision, chiefly in consequence of the pioneer work of Seligman,[1] and it has come to be recognised that even in the British Isles there were a number of economists who rejected the labour theory of value long before the time of Jevons, or indeed even John Stuart Mill. In this and other respects, valuable results have been achieved through the study of the work of " minor economists " of the nineteenth century, but much remains yet to be done.

In particular, it has normally been accepted that the early advocates of the utility approach to value did not constitute a " school of economists " in any proper sense, but were scattered in both time and place and were frequently unaware of each other's work. Yet this view may itself require some modification, for it may be contended that something closely approaching to a " subjective value school " existed at Dublin University in the mid-nineteenth century.

It must be admitted at the outset, however, that this claim involves some misuse of the word " school ", which normally connotes a body of disciples of one teacher or a group having a connected system of doctrine in common. The connection between the group of economists considered here was not so close or complete as this. All were occupants of the Chair of Political Economy in Trinity College, Dublin, but a consideration of the conditions of its tenure makes it evident that no continuous development or dissemination of any single set of principles was possible from it.

The Professorship was founded in 1832 by the Reverend Richard Whately after his appointment as Archbishop of Dublin, a position to which he came direct from the Drummond Chair of Political Economy at Oxford in 1830–31. Throughout the rest of his life Whately paid the salary of the Professor himself, examined candidates for the post, and determined the conditions of office, which he naturally modelled on those of the Oxford Professorship. The most important rules were that no person might hold office for more than five years, or be re-appointed until after the expiry of two years; and the Professor was required to deliver " a Course of Lectures on Political Economy, consisting of *nine* Lectures at the least; and also, during every year, to print and present to the Provost, Senior Fellows, and Visitors, one of such Lectures at the least. The Lectures to be free to all Graduates. Undergraduates to be recommended by their Tutors ".[2]

In consequence, as one of the first holders of the Chair remarked, " The regulations under which this Professorship is placed make it impossible for the person holding it to attempt any thing like a regular course of instruction in Political Economy ".[3] This was indeed almost inevitable in the state of economic knowledge and opinion then prevailing in Ireland, and the general and almost " popular " character of the instruction given, together with the fact that the short term of office at first attracted young graduates who usually had law rather than political economy in view for a permanent career, prevented the appearance of any strong teaching tradition. Nevertheless, the regulations had the effect, foreseen, no doubt, by Whately, of bringing a succession of fresh minds in contact with the subject and, still more important, of ensuring the publication of their views. Nor was there a complete absence of continuity in the work of these men; within a comparatively short time after the foundation of the Chair it was being occupied by individuals who had learnt their political economy from their predecessors in it. Apart from this, they were certainly aware of the views published by former Professors. Indeed, under the circumstances, the teachings of the early Professors show a striking degree of unanimity, which yet does not seem to come from any mere uncritical acceptance or expedient following of one's tenets by another.

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[1] " On Some Neglected British Economists," *Economic Journal*, 1903, Vol. xiii, pp. 335–363 and 511–535. The modified view of value doctrine cited here has been very fully and clearly developed by Miss M. Bowley (*Nassau Senior and Classical Economics*, London, 1937). It is also evident in such works as Roll, *History of Economic Thought* (London, 1938) and Whittaker, *History of Economic Ideas* (New York, 1940).

[2] *Dublin University Calendar*, 1833, p. 130.

[3] Isaac Butt, *Rent, Profits and Labour* (Dublin, 1838), p. 7. The regulations were changed on several occasions after Whately's death.

This unanimity shows most plainly in regard to value theory—not surprisingly, for whilst other subjects might be treated or ignored according to their current importance or the views of the teacher, all felt themselves at some time called upon to lay down principles about this fundamental part of Economic Theory. What is remarkable is that the principles of the Dublin Professors should so steadily have run counter to the trend of received opinion, for from the first labour or cost-of-production theories of value are either directly refuted or given a very subordinate place by them.

Whately himself is well known as an opponent of the labour theory and advocate of the utility approach, having probably been influenced in this by the views of his close friend and predecessor in the Drummond Chair, Nassau Senior.[1] Thus in his lectures at Oxford in 1831 Whately held " Labour not essential to value " and his assertion in support of this—" It is not that pearls fetch a high price *because* men have dived for them; but on the contrary men dive for them because they fetch a high price "[2]—has gained some little fame. He reiterated these views some years later when he wrote his *Easy Lessons on Money Matters ; for the Use of Young People*.[3]

This in itself provides some explanation of the line taken in value theory by the Professors of the period under consideration here. During the whole of that time, Whately assisted at the examinations for the Professorship and took a keen interest in all connected with it. Naturally, therefore, occupants of the Chair would be acquainted with his writings, and further, there is evidence that he had a personal influence on some of them.[4] But equally they were not immune from the powerful influence of accepted classical doctrine, nor was their work a mere reproduction of Whately, whose observations on value in fact constitute no more than a few striking comments. Significant as the influence of the Archbishop may have been in pointing the direction of study—and he was generally regarded as " the father of economic science in Ireland "—he can in no sense be regarded as the founder of a school or creator of a system, for his contributions to Political Economy, though remarkable, are fragmentary, and although he maintained a lifelong interest in the subject his period as a creative economist was brief.

If any one of this group of economists can be called their " Master ", it is not Whately, but the man whom he appointed to be the first occupant of the new Chair—Mountifort Longfield (1802—1884). For where Whately had thrown out some illuminating suggestions on the nature of value, Longfield constructed an extraordinarily complete marginal value theory, built upon it a Theory of Distribution, and virtually a whole system of economic doctrine.[5]

Indeed, in the work of no other member of this group of Irish economists does the theory of value bulk so large.[6] A large part of Longfield's main work, his *Lectures*

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[1] See Bowley, *Nassau Senior*, pp. 106–7; E. Jane Whately, *Life and Correspondence of Archbishop Whately* (London, 1866), references *passim*.

[2] Whately, *Introductory Lectures on Political Economy*, 2nd ed. (London, 1832), Lecture ix, pp. 252–3. These remarks do not appear in the first edition, which excluded this Lecture " as belonging more properly to a second course ".

[3] *Easy Lessons*, p. 32. This book was written in 1837 and published " under the direction of the Committee of General Literature and Education, appointed by the Society for the Promotion of Christian Knowledge ". It had reached a fifteenth edition in 1858.

[4] Thus W. Neilson Hancock, who was elected Professor in 1846, reveals that the plan of some of his lecture courses was suggested to him by Whately. (See the Preface to his published lecture of 1851, *Is the Competition between Large and Small Shops injurious to the Community?*).

[5] This is admittedly conjectural. Longfield's only other published work which can be regarded as theoretical was his *Three Lectures on Commerce and One on Absenteeism*, delivered in 1834 and published in 1835. He hints in these at further lectures, on production, to come, but these were never published. There is, however, no evidence to show that they were not delivered, for Longfield held office until 1836.

[6] An exception should perhaps be made in favour of Cairnes. But see below.

on Political Economy, delivered in Trinity and Michaelmas Terms, 1833,[1] is occupied by the consideration of it. It cannot be pretended, however, that all of this treatment is original or wholly free from confusion. At the outset Longfield makes no attempt to displace or improve the strict classical analysis, but in fact follows Ricardo in saying that " it is plain that without some utility a thing can have no value . . . But once possessed of any utility, its value will not depend so much upon the extent of that utility as upon other circumstances . . ."[2] Again, the customary classical distinction between " natural " and " market " price figures in Longfield's work; in relation to " natural " price he endeavours at great length to establish the claim of labour to be regarded as the " best " measure of value, although not attempting to make it either an invariable measure or a source of value. In this effort, as might be expected, he is indifferently successful, but in fact it is no more than a side issue in his theory of value; Longfield is much more largely concerned with exchanges and market value and it is in this connection that his theory attains its fullest development and greatest originality.

He gives a clear and attractive account of the manner in which demand and supply affect market price, with abundance of practical illustrations.[3] This is summed up in the statement " that the value of every article depends upon the demand and the supply, and that indirectly the cost of production of any commodity, as well as its utility, has an effect upon its price. The cost of production, by its influence upon the supply, since men will not produce commodities unless with the reasonable expectation of selling them for more than the cost of producing them. And the utility has some effect, although not so easily calculated, since it is to its utility, in the more extended sense of the word, that the demand is to be entirely attributed ".[4]

The further development of this proposition is so placed in the *Lectures* that it has the air almost of an afterthought, but it is really the central feature of Longfield's theory of value and his most remarkable contribution to economic thought. In a revision passage he sets out to explain more fully " the nature of demand, and its influence upon price and value ", and begins with the case of a number of " demanders " for a given article. Here he demonstrates that their demands may be of different intensities and that " the measure of the intensity of any person's demand for any commodity is the amount which he would be willing and able to give for it, rather than remain without it, or forgo the gratification which it is calculated to afford him ". Certain demands may be insufficiently intense to lead to any purchase, whilst on the other hand " there may be, and . . . in fact there generally is, an excess of intensity of demand ". Nevertheless ". . . all will effect their purchases at the same rate, viz. at the market prices . . . Now if the price is attempted to be raised one degree beyond this sum, the demanders, who by the change will cease to be purchasers, must be those the intensity of whose demand was precisely measured by the former price . . . . Thus the market price is measured by that demand, which being of the least intensity, yet leads to actual purchases ".[5]

Advanced as is this analysis, the ensuing passage is even more striking in its anticipation of marginal utility theory. In it, Longfield points out that " the intensity of demand varies not only in different places, and among different individuals, but in many cases the same person may be said to have in himself several demands of different degrees of intensity ". Illustrating this with an example of a change in the supply of provisions, he explains that " that portion which any person ceases to consume in consequence of a rise of prices, or that additional portion which he would consume if prices should fall, is that for which the intensity of his demand is less than the high price which prevents him from purchasing it, and is exactly equal to the low price which would induce him to consume it ". He invites his hearers to reason for themselves ". . . the conclusion, that each individual contains as it were within himself, a series of demands of successively increasing degrees of intensity; that the lowest degree of this series which at any time leads to a purchase is exactly the same for both rich and poor, and is that which regulates the market price ". Finally he adds: " I have chosen the example of provisions as being the most obvious and palpable, and as most frequently affording a practical instance of the principle which it illustrated; but the same observation is equally true, although not so strikingly, in every case in which a diminution of supply would occasion a diminution in any individual's consumption, without leading him to give up the use of the article altogether ”.[1]

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[1] Dublin, 1834. Reprinted in the London School of Economics Series of *Scarce Tracts in Economic and Political Science*, 1931. The pagination is identical in both editions.

[2] *Lectures on Political Economy*, p. 26.

[3] Op. cit., pp. 45–63.

[4] *Ibid.*, p. 110.

[5] *Ibid.*, pp. 111–13.

For completeness and clarity this exposition rivals the work of any of that group of nineteenth-century economists whom Miss Bowley has so aptly named “ the dissenters ”.[2] Such a statement of value theory, evolved quite independently so far as can be ascertained, would give Longfield a strong title to recognition as an economist of the first rank even were it the only novel contribution which he made to the subject. In fact, however, it is but one of a number of original views, often remarkable anticipations of later doctrines. Yet despite this, Longfield still remains definitely amongst the “ neglected economists ”. It is true that Seligman refers to him as “ in some respects the most remarkable of all ” the writers whom he “ discovered ” in 1903—true also that he has received mention from almost all historians of economic theory since that date, but the full extent and importance of his work is still scarcely appreciated.[3]

Nevertheless, history has been kinder to Longfield than were his contemporaries, at least outside his own country. On the more famous economists of the time his work had virtually no effect whatever—it was not even condemned. In Ireland, on the other hand, it was very favourably received. An anonymous reviewer (possibly Isaac Butt) in the *Dublin University Magazine* of June, 1834, had great expectations of Longfield, whom he thought had “ shown himself competent not only to understand and elucidate whatever has been written by the ablest men who have heretofore treated of the science of political economy, but to take up the subject in the spirit of an investigator by whom its fundamental principles may be more clearly ascertained, and its boundaries extended ”. “ It is our belief,” he added, “ that the living man does not exist by whom political economy may be more advantaged ”.[4]

There is evidence too that Longfield and his writings were highly esteemed in the Trinity College of his day. The *Lectures on Political Economy* remained a prescribed text for students of the subject until 1872, and Longfield himself was always spoken of with great respect by his successors, Butt placing his services to Political Economy on a par with those of Adam Smith and Lawson describing him as “ one of the ablest writers upon the Science ”.[5] It seems clear from all this that Longfield's work played a considerable part in influencing the views of his followers, and to some extent he overshadows them. None the less, the consistency and originality with which they adopted and developed the utility approach to value problems are alike remarkable.

The second occupant of the Whately Chair, Isaac Butt (1813—1879), who took office in 1836, gained lasting fame in later years through his association with the Irish Home Rule movement. As an economist, however, he is not of the calibre of Longfield, to whom he was certainly indebted for many ideas. Yet his work has considerable merit in style and arrangement, and although he has been accused of borrowing from Say and Senior, it is not devoid of originality.

On the subject of value, Butt laid emphasis on the exchange aspect, supporting the view taken by Longfield (but traceable to Samuel Bailey) that “ Value is a relation ” and criticising the idea that labour might be regarded as an invariable or "more real" measure of value—". . . to attempt to fix upon any one article as something that we will assume always bears an invariable value, is to assume, in fact, a contradiction in terms, and to lay the certain ground for errors and mistakes. The whole difficulty of Mr. Ricardo's book arises from the use of the word value in this abstract sense . . ."[1]

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[1] Op. cit., pp. 114–15.

[2] Bowley, *Nassau Senior*, p. 67. Perhaps the only members of this group whose work may be compared to Longfield's are Senior and W. F. Lloyd. It seems indisputable, however, that Longfield's anaylsis was evolved independently, for Senior's *Outline of the Science of Political Economy* was not actually published in the *Encyclopædia Metropolitana* until 1836, and Lloyd's *Lecture on the Notion of Value*, delivered from the Drummond Chair at Oxford, where he succeeded Whately, was exactly contemporaneous with Longfield's *Lectures on Political Economy*. (Lloyd's *Lecture* was published in London, 1834, and reprinted by Mr. R. F. Harrod in *Economic History*, May, 1927.)

It is curious that while the influence of Senior, acknowledged or unacknowledged, is plain in the work of most of Longfield's successors, notably Butt and Hancock, that of Lloyd is conspicuous by its entire absence.

[3] Certain parts of Longfield's work have received considerable attention, chiefly his theory of value and his observations on International Trade. The latter have been treated in some detail by Professor Jacob Viner in his *Studies in the Theory of International Trade* (London, 1937) and in his article on “ The Doctrine of Comparative Costs ” (*Weltwirtschaftliches Archiv*, 36 Band, 1932 II). There is, however, no complete treatment of Longfield's doctrines, which require to be seen as a whole with his theory of value as their foundation. The present writer was led to this conclusion, and became aware of the “ value tradition ” which forms the subject of this article, through a detailed study of Longfield's work, the results of which are as yet unpublished.

[4] *Dublin University Magazine*, June, 1834, pp. 605 and 618.

[5] Butt, *Rent, Profits and Labour* (Dublin, 1838), p. 23; Lawson, *Five Lectures on Political Economy* (London, 1844), p. 142. For a further account of these works, see below.

Miss Bowley recognises that “ In Ireland . . . Longfield bid fair to set up a distinctive school of economists ” but mentions only Butt in this connection (*Nassau Senior*, p. 109).

Butt himself was severely practical on this subject and cut through all the mazes of the labour theory to answer the question—" What is it that determines the value of commodities ? " in a single statement which few modern economists would care to contradict:—". . . their value appears to be regulated by nothing else than their utility and scarcity ".[2] There is no considerable development of these ideas in Butt's work, but he uses them as an introduction to a novel theory of original prices, based upon productivity considerations, after the method of Longfield.

The same line of thought on value was continued by James Anthony Lawson (1817—1887), who was appointed Professor in 1841, and, like both his predecessors, was a lawyer by training. His *Five Lectures on Political Economy; delivered before the University of Dublin, in Michaelmas Term, 1843*, form only part of a series and unfortunately they do not include the section, to which Lawson refers incidentally,[3] wherein the " nature and production of value " was treated. Some comments in an Appendix constitute the whole of Lawson's published observations on the subject, but these leave no doubt of his adherence to the " subjective " doctrine, and indeed explain it very lucidly within such a small compass.

Lawson holds that " It is a proposition always true, and of universal application, *that the exchangeable value of all articles depends upon their utility*, that is, upon their power to gratify the wants and wishes of man . . . Every other principle which has been assigned as the regulating cause of exchangeable value, is of partial and limited application, and subject to many restrictions and exceptions ".[4]

Explanation of this statement leads Lawson to consider the two " partial principles " commonly employed to account for the values of articles—" the proportion between the demand and the supply " and " the cost of production "—and their relation to his own general principle. In dealing with the first of these, he arrives at the concept of diminishing utility, and states it almost in so many words:—" Demand and supply *generally* influence this utility; for instance, if *A*'s supply of *X* be increased, this will generally diminish its utility to him, or the degree in which he desires its possession, for as our particular desires are capable of being satisfied, it is obvious that we may have more of an article than we wish to use, and therefore the retaining the possession of that surplus is less desirable to us ".

Lawson adds as a rider a point taken for granted by most later writers:—" the exception to this is, when the article is not one which will spoil by keeping, and when we have a prospect of being able, by keeping it longer in our possession, to obtain more of what we want in exchange for it ".[5]

The treatment of the cost aspect is almost more remarkable in its clarity of thought and expression:—". . . as a general rule, the cost of production is not the circumstance which influences a party in determining what he will pay for an article, it is, as I said, the utility of the article . . . It is not true to say that the cost of production *regulates* value, though it is true that in many cases the value, which is determined by other considerations, happens to coincide with the cost of production ". Under the customary mobility assumptions, Lawson then proceeds to show how final prices affect the movement of factors and the supply of goods, and hence concludes that " value is only made to coincide with cost of production *through the medium of a change in supply*, and when this cannot be brought about, there is no such coincidence and no tendency to it ".[6]

The quality of such passages as this makes it a matter for regret that we have not access to Lawson's main work on value, and that he did not devote himself more thoroughly to economic theory. Yet, so far as the present writer is aware, his work has attracted no attention whatsoever since John Kells Ingram wrote his *History of Political Economy* in 1888.[7]

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[1] Butt, *Protection to Home Industry; some Cases of its Advantages Considered. The Substance of Two Lectures delivered before the University of Dublin in Michaelmas Term, 1840* (Dublin, 1846), p. 125.

[2] *Rent, Profits and Labour*, p. 18.

[3] Lawson, *Five Lectures* (see note above), p. 26.

[4] and [5] Lawson, op. cit., p. 144. Italics in original text.

[6] Op. cit., p. 145. Italics in original text.

[7] There is one reference to Lawson in Gonnard, *Histoire des Doctrines Économiques* (Paris, 1930), but this refers to his views on method, not value, and appears to be derived from Ingram.

The same is true of the man who succeeded Lawson in the Whately Chair in 1846, William Neilson Hancock (1820–1888). Hancock's main interest was in social problems and what he called " applied Political Economy ". He is known as a prolific writer on the Irish economic questions of his time—his many pamphlets and reports are a mine of information for the economic historian—but he is not usually thought of as an economic theorist at all. He was, in fact, the author of only one essay properly concerned with this subject, his *Introductory Lecture on Political Economy ; Delivered in the Theatre of Trinity College, Dublin, in Trinity Term, 1848* ".[1] This, however, provides one of the most interesting discussions of value theory given by any member of this group of Dublin economists, a discussion particularly noteworthy for its unusual anticipations of modern views.

Hancock begins by pointing out the ambiguity of the word " value ", adding that " this ambiguity about value has been increased by some economists using the word as if it were synonymous with cost ". But—" Now the word ' price ' is fortunately free from all ambiguity, and always means the exchangeable value of a commodity, estimated in the money of the country where the exchange takes place ". Consequently, he says, " I propose to substitute observations into the variations in price, for observations into variations in value ".[2] The manner in which Hancock sets about doing this is admirably in accord with the methods of modern price theory. He explains that there may be two types of price changes—the familiar " changes from the side of things " and " changes from the side of money " or, as he calls them, " intrinsic and extrinsic changes in prices ". The parts of the subject dealing with these " may conveniently be called—the former, the theory of prices, and the latter, the theory of money ".[3]

" To proceed then, with the theory of prices, it is found by observation, that no intrinsic variation takes place in the price of any commodity, except from one of two causes; either a change in the degree in which its possession is desired, or in its desirability; or a change in the force of the causes by which its supply is limited, or, in other words, by which it is made scarce."

In developing this statement, Hancock explains his concept of " degree of desirability " by reference to the collective demand of the market:—

> " The degree in which the possession of a commodity is desired, is measured by the number of persons able and willing to purchase at each amount of price. And it is observed that for commodities in general, their desirability increases very rapidly as their prices fall."[4]

At first sight, this phrasing might suggest that Hancock's " degree of desirability " is something very different from Longfield's " intensity of demand " and the normal concept of diminishing utility. In fact, Hancock's approach through aggregate market demand has, as it were, carried him past diminishing utility towards the consequent idea of elasticity of demand, although he does not succeed in expressing this very clearly. Having cited the penny post as an instance of his point, he proceeds without further elaboration of it to indicate the relative importance of demand and supply factors in a passage which inevitably puts one in mind of Marshall:—

> " As very few commodities are capable of an instantaneous increase in supply, the intrinsic variations in prices for short periods depend almost entirely on the changes in the desirability of commodities. But as most commodities can be increased in supply, the permanent intrinsic variations in prices depend almost entirely on changes in the degree of their scarcity."[5]

This leads Hancock to consider the influences affecting the supply of goods, and of these he says:—" The causes, then, which limit the supply of any commodity arise from the necessity of using raw materials and the three instruments of production; and the force of these causes is estimated by the sums which have to be paid for their use, or, in other words, by the respective prices of the materials and of the use of the instruments of production ". From the supply side, then, the price of a good depends on the amounts of factors and materials required for its production, multiplied by their prices, and, following out his general principle, Hancock treats the question of the returns to the factors as entirely a pricing problem. Referring to the theories advanced to explain these returns, he says:—

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[1] Dublin, 1849.

[2] Hancock, *Introductory Lecture*, pp. 9 and 11.

[3] *Ibid.*, p. 13.

[4] *Ibid.*, p. 14.

[5] *Ibid.*, p. 14. Compare Marshall, *Principles of Economics*, 8th ed., Bk. V, Chap. III, p. 349. The contrast between Hancock's emphasis on scarcity and Marshall's on cost of production is noteworthy. Cf. Cannan, *Review of Economic Theory*, p. 212. And see below (p. 146).

> "The method of commencing the investigation of these theories is the same in all; namely, by the application of the laws already stated with regard to other prices. The prices of labour, of savingness, and of the assistance of natural agents depend on the causes which make the use of labour, capital, and natural agents desirable, and limited in supply, or scarce."[1]

Naturally, no detailed explanation of these views is undertaken by Hancock in this single lecture. How he may have developed them is a matter for conjecture, and in the light of later knowledge there is some danger of overestimating their depth and significance. Yet it does not seem too fanciful to suggest that here, in 1849, there appears at least the germ of a general interdependence theory of pricing. Certainly it is a theory which, with its emphasis on price instead of value and scarcity instead of cost of production, diverges notably from the classical approach, and in essentially the same fashion as do the doctrines of such modern economists as the late Professor Cassel.

Nevertheless, at the time of Hancock's resignation of the Whately Chair, in 1851, the classical, or more precisely the Ricardian, system was still dominant. Modified and improved, it was gaining new strength and prestige from the publication of J. S. Mill's *Principles*, the influence of which far outweighed and overshadowed that of Longfield's *Lectures*, which had never reached a second edition.

It is scarcely, surprising, therefore, that there appears at this period a reaction from the trend which value theory had been taking at Trinity College. Thus Richard Hussey Walsh (1825—1862), who took Hancock's place in 1851, appears to have accepted the view that value depends upon cost of production. His main work, however, was an *Elementary Treatise on Metallic Currency*[2] and the comments on value in it are so few and so limited in application that it is difficult to speak with any certainty about the true character of his ideas. He gives "a general statement and explanation of the principle that the value of goods is regulated by their cost of production" but says that "it is subject in its application to many limitations". Most important of these is his statement that "the influence on value of the cost of production is gradual and indirect, being chiefly brought about by the effect exercised on the supply".[3]

This would seem at first to take the argument somewhat away from the true cost approach, but in reality it seems only to be a use of the term "value" as equivalent to classical "market price". On the whole, Walsh appears to give the demand aspect of value questions a subordinate place, but he deserves credit for setting out more fully and clearly some of the ideas on demand on which Hancock had touched, for at one point he remarks that ". . . if, from a considerable augmentation in the supply of a commodity, its value falls, the decline will be more or less rapid according to the relation which the commodity occupies with respect to human wants . . . in order that a considerable augmentation of supply should diminish value but slightly, the article should enter very generally into consumption, and be one for which the demand might rise rapidly with every reduction in price".[4] This promising line of analysis is not continued, however, beyond the point necessary for its application to the question of variations in general prices occasioned by gold discoveries. Indeed, all Walsh's comments on value are intended for use in the study of monetary problems, and in this respect their emphasis on the supply side is natural; yet there seems little justification for regarding him as anything but a supporter of the orthodox doctrine.

Whatever doubts may be entertained as to the position of Walsh, there can certainly be none as regards the views of his successor, John Elliott Cairnes (1823—1875). Cairnes is generally regarded as the "last of the classical economists" in the strict sense, and, in value theory at least, this appears to be one point on which popular views require no modification. The first of the Dublin Professors to devote his life primarily to economic study and teaching, Cairnes remained throughout his entire career a rigid adherent of the cost-of-production theory of value. It is true that his formulation of it was very different and very much more cautious than that of Ricardo or McCulloch. Yet the greatest change which he made in it was to admit that value was determined by reciprocal demand where the mobility assumption was not fulfilled for factors of production. The concept of non-competing groups which he developed in this connection has found an accepted place in economic theory, and is perhaps the best known part of Cairnes' work. In fact, however, the idea had been fully stated by Longfield,[1] not in relation to the theory of value, but in connection with the theory of wages, where it properly belonged. This was never recognised by Cairnes, who very rarely took any account of the work of his Irish contemporaries on economic theory. The " lack of intellectual sympathy " for the views of others, which shows so plainly in his scathing criticisms of Jevons and the critics of the classical system, is evident here in another form. One cannot but wonder whether, and how, Cairnes reconciled his theories with those in Longfield's *Lectures*, which were prescribed for reading by the students he examined.

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[1] Hancock, *Introductory Lecture*, p. 20. "Savingness" is Hancock's rather clumsy substitute term for Senior's "Abstinence".

[2] Dublin, 1853.

[3] *Elementary Treatise*, p. 19.

[4] Walsh, op. cit., pp. 77 and 79.

At this stage it would certainly appear that the trend which had been so well started by Longfield had been completely reversed after the lapse of twenty-five years. No compromise on value problems was ever essayed by Cairnes, either as a concession to his predecessors in Ireland or his successors in England. Yet in Trinity College this conflict eventually produced one noteworthy result—a " dual theory of value " in which the opposing doctrines were attempted to be synthesised. This was the work of Arthur Houston (1833—1914), who followed Cairnes as Whately Professor in 1861, and published his views under the title of *The Principles of Value in Exchange, explained and expressed in simple and comprehensive formulæ. Two Lectures Delivered in the University of Dublin.*[2]

In this short work, starting from the assumption that the commodities exchanged are produced under completely free competitive conditions and " are naturally of the same degree of utility ", Houston reaches the conclusion that " the value of freely-produced commodities is regulated by their Net Cost of Production ".[3] Developing this, he proceeds to explain that " Net cost of production is the *power* by which value is regulated: *limitation of supply* is the *lever* by which that power is applied. If any force interferes to prevent the free movement of the lever, to prevent its obeying the impulse received from net cost of production, the power fails to produce its proper effect, and value diverges from its natural course "[4] Next Houston abandons the " equal utility " assumption and considers how the value of commodities will be affected by their comparative utility when taken together with the possibilities of varying the supply, concluding:—

> " To sum up, when commodities, the subject of exchange, require some time before a new supply can be procured, scarcity, actual or apprehended, will give rise to a deviation from natural value, greater or less in proportion to their utility, or, in other words, in proportion to the imperiousness of the desire they are adapted to satisfy."[5]

With commendable analytical thoroughness, Houston also removes his assumption of competitive production, and thus carries the study of value a stage further than any of his predecessors before Cairnes by taking in the question of monopoly value also. His treatment of this is especially interesting in the light of modern work, by reason of his recognition of the influence of conditions of demand on the conduct of the monopolist. He points out that beyond a certain point the monopolist by increasing output must " lower the price against himself ". " It must not be supposed, however, that such will necessarily prove a losing speculation for him, as it is possible —nay, probable—that his gains on the larger quantity at the lower value, would be greater than those on the smaller quantity, at the higher." In an Appendix to this, Houston adds: " It is perhaps worth while to inquire what this depends on. It depends, *ceteris paribus*, on the rate at which value falls with increase of supply, in other words, on the extensibility of the demand ".[1]

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[1] Longfield, *Lectures on Political Economy*, pp. 83–4. This point has lately received recognition in Whittaker, *History of Economic Ideas* (1940), p. 601.

[2] London, n.d. The exact date of this work is difficult to ascertain. Professor J. G. Smith, in his article " Some Nineteenth Century Irish Economists " (ECONOMICA, New Series, Vol. II, p. 30, Feb., 1935) gives the title with the addition *being Two Lectures Delivered in the University of Dublin, 1864*. The copy in the Library of Trinity College has the title given in the text here, but the dedication as a tribute to Whately describes the lectures as " the last delivered in his lifetime from the chair he founded ". This would make the date *of delivery* 1863, and this is therefore the date chosen as marking the end of the period with which this paper deals.

[3] Houston, *Principles of Value*, p. 22.

[4] *Ibid.*, pp. 29–30. Italics in original text.

[5] *Ibid.*, p. 36.

So here at last that notion about demand which Hancock and Walsh had expressed finds an integral place in value analysis and a name,—a name extraordinarily like that by which it is known to every student of Economics to-day.

In general, there is a noteworthy resemblance between the theory of Hancock and that of Houston, although the latter was certainly strongly influenced by Mill. Like Hancock, Houston emphasises the importance of demand factors in the short period as compared with supply factors in the long, but in Houston's version the emphasis on supply factors—again represented by " cost of production "—is much stronger, the utility aspect being almost incidental. Indeed, so strongly is he imbued with " real cost " ideas that he goes far beyond the classical school in the endlessly disputed matter of measuring values " in real terms ". Making the customary admission that it is impossible to find an invariable measure of value, Houston yet proposes a measure of cost of production in terms of sacrifice " which should enable us to determine the relative values of freely-produced commodities so far, and so long, as the conditions of their production did not vary ". To this end he endeavours to construct a " unit of sacrifice " and even goes so far as to give a hypothetical example of its use, couched in mathematical terms.[2] Criticism would be superfluous, but, despite such defects, Houston's doctrine can bear closer examination at the present day than Cairnes'.

Houston vacated the Dublin Chair in 1866, when the time of the general recognition of the " subjective " approach to value was at hand. As the change is customarily depicted, the work of Cairnes appears as the last statement of the old doctrine, that of Jevons and Menger as the first statement of the new. It is true, indeed, that Cairnes expressed the dominant theory of his day, but equally it appears that his views were more or less alien to the trend of economic thought of the University where he graduated and the Chair he first occupied. Yet perhaps it is true to say that on value questions the work of the early Whately Professors did reflect the general trend of their time. For, after all, in a reflection things *are* reversed.

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[1] Houston, *Principles of Value*, p. 44, and Appendix C, p. 90.

[2] Houston, op. cit., Appendix B, pp. 86 *et seq.* In justice to Houston it must be said that he gave other lectures on " value in use ", which he did not publish, but which may have given a more balanced picture of value theory as a whole.

Professor Hayek's Philosophy

By A. H. Murray

In a series of articles the importance of which cannot be overestimated for the theory of method of economics, entitled *Scientism and the Study of Society*,[1] Professor Hayek offers what may legitmately be called a philosophic criticism of the tendencies towards " planning " and " engineering " in contemporary economic theory. The social sciences have grown up almost surreptitiously, catching the philosopher unawares, so that he has not, generally speaking, turned his critical and analytical activities on these branches of knowledge and the methods employed by them to any great extent and certainly not to the extent to which he has devoted his attention to the methods and subject-matter of the " natural " sciences. Also the separation which grew up, especially during the nineteenth century, between philosophers and scientists when philosophers lived in ivory towers—according to an eminent historian[2]—and which has been continued in the twentieth century wherein scientists have retired into the ivory towers of mathematical abstraction, has tended to withdraw the social sciences from the sphere of philosophy. So it has come about that the social sciences have missed the analysis and criticism of " scientific " method which philosophy has turned with good effect on the physical sciences and have missed the clear indication of its usefulness and limitations with which philosophy has been able to assist the natural sciences—and which has generally been accepted by these. In these articles, however, the social scientists are being driven by the sheer logic of facts to see what the philosopher could have told them all the time. For the philosopher the significance of Professor Hayek's articles is not in the first case to be found in the cogency of his plea for unplanned society. It rather lies in the fact that Professor Hayek examines scientific method to discover its usefulness for the study of a particular subject-matter: in this case social phenomena.

It is our purpose to indicate some of the main implications which appear to be involved in Professor Hayek's argument which have a bearing on theories of the method and the subject-matter of the social sciences. Since the articles appeared some time ago we will summarise briefly not Professor Hayek's argument for unconscious purposiveness in social action (which is the burden of his message), but those aspects of it to which we will have to refer particularly.

I

The line of argument which Professor Hayek adopts with regard to the three schools of tendencies in economic thinking which have arisen as the result of the uncritical use of argument from analogy with the natural sciences, viz., objectivism, collectivism and historism, may be called, for want of a better term, psychological. The characteristic of objectivism, says Professor Hayek, is that it tries to dispense with our subjective knowledge of the working of the human mind. But we cannot get rid of the human mind even in the study of physical facts, much less so when the fact we study is a social situation. All mental phenomena must be regarded as acts of classification performed by the brain. There is, however, this difference involved when we study external nature and when we study society; in the former case our sensations and thoughts are not links in the chain of discerned events (they are merely about them), while in the latter case these sensations and thoughts form an essential element which goes to give the situation the character it has. So that even if we should one day be in a position to explain how the brain transforms things into ideas that knowledge would still not make an " objectivist " method possible in the study of social phenomena. Professor Hayek concludes with some wise and timely words on the uncritical tendency to seek a quantitative expression for knowledge, and the limitations thereof. It is to be noticed that the argument against objectivism assumes the sharp distinction between object-out-there and idea which is characteristic of modern psychology—but has not always been accepted in psychological teaching by the great minds of our tradition.

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[1] Economica, Vol. IX, No. 35, p. 267; Vol. X, No. 37, p. 34; Vol. XI, No. 41, p. 27.

[2] Charles Singer, *A Short History of Science*, p. 389.

Professor Hayek's criticism of collectivism—the second school—continues the analytical approach. He very rightly argues that social phenomena such as " capitalism ", " society ", " economy ", which are too easily regarded as observed or given wholes-out-there by the social scientist, cannot be known or indicated until they are studied from the point of view of a knowledge of the minds of the people whose actions go to constitute the social wholes. That is, they must be studied from the inside, so to say, and not merely from the outside, for they are the products of the minds of men. Thus these conceptual entities must not be isolated from the minds of the people who think them as if they exist independently of these minds. " The social sciences, thus, do not deal with " given " wholes but their task is to *constitute* these wholes by constructing models from familiar elements," writes Professor Hayek.[1] And again: " We do not first observe that the facts always occur together and afterwards ask what holds them together; but it is only because we know the ties that hold them together that we can select a few elements from the immensely complicated world around us as parts of a connected whole ".[2] Thus the social scientist has to do with a world of " constructs " or ways of thinking. And again Professor Hayek concludes his analysis with important remarks on the use of statistics in the study of society.

The psychological argument is maintained in the analysis of historism. In a scholarly and penetrating analysis Professor Hayek points out that historism in the sense that scientific method can be applied to the study of historic data was created not by historians but by students of the social sciences. Yet clearly the historian is primarily interested in the uniqueness of a particular situation while the natural scientist seeks to lose the uniqueness in a set of laws holding for numerous situations; the unique character of a situation as complex as an historical event can only be discovered by a careful selection of the facts and the art of selection involves the principles of selection. Thus history involves theory, for the selection of the facts will be guided by the question asked. History is always the reply to a question; it is a construct, for though the historian may use as elements only what he finds empirically, not observation but theory will tell him which of these elements are converted into a unique whole. So Professor Hayek makes short shrift with the " philosophies of history " and with the type of doctrine which teaches that social phenomena can be understood only in terms of the categories of history, in a particular historic context. He points out that the phenomenon of monopoly consists of certain fixed relations between individuals, has always consisted of this, and can only be understood " from the inside ", that is, from the angle of men's attitude of mind to each other.

Professor Hayek is fully aware of what his argument involves. " To recognise the existence of a mind," he writes towards the end of his criticism of the three scientistic schools, " always implies that we add something to what we perceive with our senses, that we interpret the phenomena in the light of our own mind, or that they fit into the ready pattern of our own thinking ".

A second aspect of Professor Hayek's argument against theories of a planned society which is significant for the theory of method is contained in his statements on undesigned ends in society. It is because man in society " constantly promotes ends which are no part of his intention " (the author quotes Adam Smith) that the attempt to apply the categories and the method of the natural sciences to the study of social phenomena must fail, and therewith all hope of a planned society. The subject-matter of the social sciences cannot be classified into the categories of the natural sciences for the social sciences have to do not merely with the relation between things but with the relation between man and things and man and man. What these relations are we cannot observe; we can only infer it from our knowledge of our own minds, on the assumption that all people's minds work the same way. Thus the material of the social scientist is subjective, not in the sense of being merely relative to the observer but in the sense of being of the nature of mind, and so not directly externally observable. To handle this material a new method is required, which Professor Hayek calls the individualistic or compositive method.

II

An outstanding and valuable feature of Professor Hayek's articles is his description and analysis of method. Two points occur, both relating to the differences, apparent or otherwise, in method between the social sciences and the natural sciences.

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[1] X, No. 37, p. 44.

[2] *Ibid.*, p. 46.

Firstly, it is sometimes said, and popularly believed by people, that one of the marks which distinguishes a natural science from other organised forms of knowledge which have been attained in a methodical way is the method which is peculiar to the natural sciences. Now Professor Hayek does say that there is a profound, even a fundamental, difference between the methods of the natural and the social sciences (and a discussion of this is our second point) but he does not say that this difference is original, in the sense that it is it which distinguishes the two groups of study. The difference in method follows on a difference further behind: in the subject-matter of the two studies. The social sciences have to do with the relation between men and things, and men and men, and not with things out of their relation with the men who study them. These relations are mental, being the opinions and beliefs held by people, so that the social sciences deal with the phenomena of the individual mind. From these characteristics follows the method peculiar to the social studies. It is only what people know or believe that can enter as a motive into their conscious action and it is the task of the social scientist to follow up the implications of these beliefs and knowledges. In this way he will be able to reconstruct the patterns of social relationship and to see " the unintended and often uncomprehended results of the separate and yet interrelated actions of men and society ". The process is a synthetic or compositive one. These patterns or wholes which form the peculiar subject-matter of the social sciences have the further characteristic of being unique and individual. Herein, too, the social sciences stand in contrast to the physical sciences wherein the particular event is usually one of a very large number of similar events, so that the " law " of the event is achieved by a process of abstracting the element common to all the cases and casting off individual and distinctive characteristics. So Professor Hayek describes the method of the social sciences as the individualistic, compositive method. He writes: " From the fact that it is the concepts and views held by individuals which are directly known to us and which form the elements from which we must build up, as it were, the more complex phenomena follows another more important difference between the method of the social sciences and the natural sciences. While in the former it is the attitudes of individuals which are the familiar elements and by the combination of which we try to reproduce the complex phenomena, the results of individual actions, which are much less known—a procedure which often leads to the *discovery* of principles of structural coherence of the complex phenomena which had not been (and perhaps could not be) established by direct observation—the physical sciences must needs commence with the complex phenomena of nature and work backwards to infer the elements from which they are composed. The place where the human individual stands in the order of things brings it about that in one direction what he perceives are the comparatively complex phenomena which he analyses, while in the other direction what is given to him are elements from which more complex phenomena are composed which he cannot observe as wholes. While the method of the natural sciences is in this sense analytic, the method of the social sciences is better described as compositive or synthetic. It is the so-called wholes, the groups of elements which are structurally connected, which we learn to single out from the totality of observed phenomena only as a result of our systematic fitting together of the elements with familiar properties, and which we build up or reconstruct from the known properties of the elements ".[1]

It is clear that Professor Hayek infers the method to be used from the nature of the subject-matter. What is not quite clear—and this brings us to our second point—is whether the method as Professor Hayek describes it really is as different from the method of the natural scientists, as he suggests, and whether he would not, by a further probing, have discovered a deeper difference of methods.

It will occur to the reader that Professor Hayek has somewhat over-emphasised the analytic aspect of the procedure of the natural sciences and has tended to overlook the analytic procedure which is present at least as a preliminary in the process by which the social scientist handles his material. The natural scientist certainly does not stop in analysis. Having analysed his datum into its constituent elements, the relations between which he expresses in some fresh, nonsensory form—for example, mathematical—he promptly proceeds to reformulate the object in a total comprehensive mathematical formula—sometimes called a "law". There seems, indeed, to be a difference between the method of the scientist and of the social scientist, but it seems to be more than the difference of the predominance of analytic procedure over against synthetic procedure. A comparison with a description of method by Hobbes will, I think, make it clear that Professor Hayek's description of method has not yet got to the real difference between the two methods, that of the natural and that of the social scientist. Indeed, it will appear that Professor Hayek has himself in this respect not quite escaped from the trammels of scientism—or, as we will call it further on, cartesianism.

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[1] IX, No. 35, pp. 286–7.

Hobbes, it should be remembered, was a contemporary of Descartes and spiritually a cartesian, and as a son of his times had a good deal to say on method of study and what knowledge consisted of. It would appear that Galileo had already used the term "resolutive-compositive" for the method which he used[1] and Hobbes admittedly copies the method from Galileo. He explains his method as follows: "For everything is best understood by its constitutive causes. For as in a watch, or some such small engine, the matter, figure, and motion of the wheels cannot well be known, except it will be taken insunder and viewed in parts; so to make a more curious search into the rights of states and duties of subjects, it is necessary, I say, not to take them insunder, but yet that they be considered as if they were dissolved". Hobbes then goes on to show how he applied this method to the study of society and how, having reduced the phenomena of society to their elements, he is able to reconstitute the wholeness of society by processes which he calls deductive but which differ little formally from the synthetic or putting together process which Professor Hayek calls compositive. Hobbes writes: ". . . that is, that we rightly understand what the quality of human nature is, in what matters it is, in what not, fit to make up a civil government, and how men must be agreed among themselves that intend to grow up in to a well-governed state".[2] And so Hobbes takes the elements of society, which *inter alia* include the state of nature and its equality of right unto all things, the compact, rights, dictates of reason, and so forth, *reduces them to their bases in individual wills, and then re-composes them into the whole called the state*. The state is an individual whole in one sense, for it has its own characteristics and implications and it contains its own reason and deduction in itself. The merit of this method is that it contains the guarantee that no extraneous matter will intrude itself, for is it not from the very elements of the object that the whole is reconstituted? The value of the method for Hobbes is its applicability; this is in the spirit of the times.[3] The compositive method of Galileo in the hands of Hobbes makes of political philosophy a technique for the administration of the state. The method also produces the politics for a planned state, so that it appears that a further distinction is necessary in the theory of the social sciences which would make it peculiarly suitable for the study of a society which is essentially unplannable. The reconstitution of the whole leads Hobbes to a mechanistic society which Professor Hayek wants to avoid at all costs for he appreciates the connection between a mechanistic political philosophy and the state based on force and surrender of the individual will, of which Hobbes is the exponent. Mr. Strauss has shown conclusively that what leads Hobbes to his mechanistic reconstruction and his political science is not in fact his method, but something that went before. The method was clearly adopted later. The new moral attitude, which broke from the traditional view in which man had been seen in relation to the cosmos, exemplifies the cartesian dismemberment in political theory, for it sets man up over against his fellow men. This attitude Mr. Strauss has pointed out was closely associated with Hobbes's interest in mathematics and the natural sciences but is "objectively as well as biographically 'prior' to the mathematical scientific founding and presentation of that philosophy". It, is, in fact, Hobbes's psychological premises—man's fear of death and his fundamental rapacity—which lead him to the conclusion of the mechanistic state and political technique. The problem is clearly a matter of premises, and not merely one of method. If Professor Hayek expects his individualistic compositive method not to lead him into a planned, mechanistic view of society he must examine carefully the premises involved in his theory of the nature of his subject-matter, that is, "the concepts and views held by people". Method alone is not sufficient. The compositive method by itself is still too near cartesian scientism, which denied any place to the idea and to how people think in the study of phenomena. The synthetic or compositive process with nothing more to it may still lead to a mechanistic view of society. The fact is that Professor Hayek has reintroduced an historically very important element into his conception of social phenomena, in the shape of the constitutive idea. He has gone back to before Descartes prepared the way for (and himself committed) scientism by separating *res cogitans* from *res extensa*, thereby giving priority to *res extensa* and the methods suitable for the study of *res extensa*. Further probing by Professor Hayek into the processes really used by students of social phenomena will bring him to the insight that these consist in fact of the processes regarded before Descartes (and since) as particularly suitable for the study of things with which ideas are mixed up, and usually called philosophic analysis or the analysis of the concept.

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[1] Dedicatory Epistle to *Philosophical Rudiments concerning Government and Society* (Molesworth's trans.), II. p. vi.

[2] *Loc. cit.*

[3] Leo Strauss, *The Political Philosophy of Hobbes* (Oxford), Ch. viii.

III

Professor Hayek blames the excesses to which the uncritical enthusiasm for scientific method has gone in the nineteenth century and it is true that we are to-day taking the exuberant optimism of that great period—on matters of politics as well as science—at a more critical estimation. It will help to bring out the significance of Professor Hayek's argument if we refer scientism to its origins further back in the history of thought: to the attitude of mind which we have inherited from the great centuries of Copernicus, Kepler, Galileo, Tycho Brahe, Descartes, Hobbes, Newton. We will refer to this attitude of mind, and the theory of knowledge of these people, as cartesianism, for Descartes was after all the philosopher who gave the clearest philosophical expression to them. But we will write cartesianism with a small c to show that Descartes must not himself be held responsible for all the things scientism did. It will become clear that Professor Hayek's argument is an indication of the failure of the cartesian starting-point when it comes to the study of social phenomena. The introduction of the idea into social phenomena by Professor Hayek marks in fact a reaction against the prevailing cartesianism of our time which has also invaded the social studies. It was Descartes who created the dualistic psychology in which the over-sharp distinction is made between those parts of experience commonly known as things-out-there and that part of it called ideas or being-aware-of, which gave the problem of certainty and knowledge a form more unreal than it had had in the less abstract and more empirical psychology of Aristotle and the Middle Ages. When Descartes made the sharp distinction between *res extensa* and *res cogitans* he also did a great many other things, some of which he probably did not want to do. He created the impression that mind was a thing apart from the things of which it was aware and that it had its own peculiar machinery which could be observed, so that we could describe " how it worked ". Ideas became entities almost like mental atoms which mind—or, rather, the brain—somehow juggled round and if they came out in a certain way the owner of all these ideas was " certain " about what he knew. Later cartesians tended to identify mind with brain, or at least, to regard mind as a function of brain. This soon led the cartesian dualists to make two assumptions: the one was that one could study the mind by analogy with the physicists and biologist's categories; and the other that an idea was " in the brain ", and so private and subjective, so that one never can be quite certain that the other person thinks as one does. One has to infer it from their behaviour. Cartesianism is the philosophy of dismemberment. It separates what in ordinary experience seem to belong together.

It is against this dismemberment that Professor Hayek in fact protests when he restores the idea to its place in the social phenomenon. So much is clear. What is not so clear is whether Professor Hayek is aware of the full implications of this restoration. It rather seems that Professor Hayek retains cartesian psychology—or at least the terminology and modes of expression of cartesian psychology—and that this leads him into a typical and inevitable cartesian dualism, and that this is unnecessary and would be avoided if he used a philosophic or mind-psychology which is what his argument involves.

Professor Hayek adopts cartesian psychology—or at least the modes of expression of cartesian psychology—when he writes that " concepts or ideas can, of course, exist only in individual minds "[1] and that ". . . all mental phenomena . . . must be regarded as acts of classification performed by the brain ".[2] The assumption that the mental world of each person is private and particular shows itself when Professor Hayek is concerned with his criticism of the method of " objectivism " and is faced with the task of explaining how we know that other people know that we know and we that they know. " It is true, of course, that we know nothing about other people's minds except through sense-perceptions " . . . We know that other people like ourselves regard any of a large number of physically different things, a, b, c, d, . . . etc., as belonging to the same class; and we know this because other people, like ourselves, react to any of the things by any one of the movements α, β, γ, δ. We continually and successfully act on the assumption that we do classify these things in the same manner as other people do ",[1] he writes and then goes on to discuss the hypothetical probability that one day we may be able to substitute physical objects for the mental categories which we use nowadays, when, presumably, all the problems of the privacy and subjectivity of our knowledge will be solved (although " meaning " would still be mental). There is here no appeal to the " universal " in any way; in true cartesian spirit knowledge of minds is referred to sense-perception and so is created the problem of particularity and subjectivity which worried Berkeley and Hume. When Professor Hayek tries to account for the fact that we expect other people to act as we do his appeal is not to any " insight into " or " apprehension of " the nature of the situation and its internal logic, but to perception: " Our procedure is based on the experience that other people as a rule . . . classify their sense impressions as we do".[2] The important question why it is that the classification which all these people happen to make in the same way should work, and the implications of this question, are not discussed.

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[1] IX, No. 35, p. 284.

[2] X, No. 37, p. 37.

On the basis of this cartesian assumption of the privacy and particularity of ideas Professor Hayek now has to describe those realms of experience which are the peculiar subject-matter of the social sciences. Professor Hayek remains true to the logic of his psychological assumption. The social sciences study " the views and concepts that people hold," we are told, for it is these things that determine the action of people, where by views and concepts is not meant their knowledge of physical nature, but all things that determine their ways of behaviour, that is, their mental life. In distinction from the natural sciences, which study the things-out-there, eliminating the personal factor, the social sciences study the patterns or wholes of behaviour—for the behaviour of men form wholes—" from the inside ", that is, from a knowledge of the attitude of mind of the individual or, if you like, the ideas the individual has about them. To understand the whole is to understand this for the various social wholes which we assume to exist (such as capitalism, society, etc.) and on which assumption we act. They have the shape and structure of the individual mind. Indeed, we are told that the social sciences do not deal with " given " wholes, but have the task of " reconstructing " the wholes from a knowledge of the constitutive ideas. Thus it is bad method in the social sciences to start by studying " the nation ". The notion of nation contains what men think it is, and thus it is the ideas in the minds of men that must be studied. From these ideas the notion of nation must be " reconstructed " by the social scientist. So wholes such as government or trade or army or knowledge " do not exist for us apart from the theory by which we reconstitute them, apart from the mental technique by which we reconstruct the connections between the observed elements and follow up the implications of this particular combination ".[3]

The difficulties inherent in such a construction-view of social theory on the basis of cartesian or atomistic psychology are obvious. We may illustrate these by reference to Professor Hayek's remarks on history. Professor Hayek rightly points out that history on his view becomes the question we ask of it—for our ideas form a constitutive element in history. On a cartesian theory of ideas, however, it becomes exceedingly difficult to offer any criterion as to when the question we ask of history is meaningful or meaningless, for ideas are " in the brain ", particular and private. The sense or nonsense of a question asked by the historian can depend only on one or both of two inherent characteristics of the ideas involved: their bearing on objective reality, and (or) the logical coherence made possible in the answer by the form of the question. The trouble with cartesianism is that it can account for certainty or knowledge only on a theory of clear and distinct ideas, that is, on mathematics, and Professor Hayek has indicated the limitations of mathematical expression. But he does not offer any description of the nature of constitutive ideas which suggests that they are able to meet either of the two above criteria: there is no mention of any logical coherence in ideas, much less of any quality such as universality so that we may know that what I have experienced or thought as " history " is also what other people have experienced or thought, or is history. Cartesianism does not allow for this kind of thing; it cannot, on an atomic theory of ideas in the brain, of a brain which does the classifying, and of the privacy and particularity of ideas which essentially is the separation of the known object from the knowing mind. That is why it produced scientism. The attempt to cover up this problem and to re-introduce the idea into the phenomenon by the use of the merely descriptive phrase " similarity " of ideas of various people will not work (and in any case the concept of similarity contains metaphysical problems which cannot be met on a cartesian premise).

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[1] X, No. 37, p. 36.

[2] IX, No. 35, p. 277.

[3] X, No. 37, p. 56.

It is significant to note that cartesianism will run into dualism even when only cartesian expressions and terminology are used—and the reader must remember that we are not accusing Professor Hayek of being a cartesian (indeed, our purpose is to show that his argument really involves a different psychology) but only of unfortunately using cartesian terminology. Professor Hayek points out that the thinking or ideas of the person form an internal part of the subject-matter of social studies. Then he goes on to warn the reader that really there are two sets of ideas in the mind of the person. The first set or class is those ideas of which the social phenomenon or whole (society, capitalism, economics) may be said to exist, and this class of ideas is properly the subject-matter of the student of social phenomena. The other class of ideas, which must be distinguished from this, consists of the ideas and opinions of people about the first set of ideas; they are the theories which I may hold about the phenomena, while the first class of ideas makes or bring about the social phenomena and is called constitutive ideas. The separation of *res extensa* from *res cogitans* is back in the form of the separation between " constitutive ideas " and ideas about these.

But this dualism in the mind raises qualms. It may on occasion be useful to distinguish between the two so-called kinds of ideas as a warning to a young student in training to distinguish between " opinion about " and " constituent fact ". But if the distinction is maintained, as it must be on a cartesian psychology, it must lead to an infinite regress, for (i) who is to say that the " idea about " (i.e., the second class of ideas) is not really the constituent idea, for there is no distinguishing mark inherent in the idea as such to indicate which set of ideas is which so long as ideas are regarded as particular and private; and (ii) the fact is that there are not merely two classes of ideas in the mind, for the moment I have an opinion about anything I have an opinion about my opinion about it, and so forth, and who shall say (on a cartesian psychology) that the opinion about of the *n*th degree is not really the constituent idea influencing my behaviour. The fact is that what Professor Hayek here describes as a difference between kinds of ideas is really the distinction between the process of reflection or the activity of the idea, when my mind plays round the main idea and, in playing round it, changes it critically, and the constituent idea (the expression is apt) which is the idea which guides my behaviour in this connection. The fact also is that there is a good deal more to an idea which is really a constituent idea than appears from Professor Hayek's cartesian description of it. It will be readily admitted that social wholes seem to retain their identity in spite of considerable change in opinions held by people about them. It seems that there must be something more to constitutive ideas than merely being ideas of a certain class in a person's mind. If the constitutive ideas are to be ideas of real wholes—that is, wholes that hang together in some form of coherency without internal contradiction, and, since they are social phenomena, wholes in which other people participate, so that their behaviour does not clash with mine—there must be a kind of common objective quality about them perhaps best described as a core of internal logic; and when this quality is common to all the constitutive ideas of the people who share in this whole it is commonly called " a universal ". To the extent that this is absent various behaviours *vis-à-vis* that whole will (and do) conflict and there is no real social whole. Further, it is clear that the idea from which the reconstruction of the social whole has to take place and which motivates men's behaviour so that wholes such as government, companies, social systems are formed necessarily must have two aspects to it if it is to be a truly constitutive idea. It is idea-as-idea; and it is also idea-as-act, otherwise there can be no relation between it and the behaviour of the individual. As idea the idea is in the consciousness of the individual; as act it is expressed or embodied in behaviour and in the relations which behaviour lays between space-time objects. In that sense we may say that the idea is embodied in space-time objects, or, in social phenomena. There is no *a priori* reason why the careful examination of the idea as act (that is, of external behaviour or form)—in the shape of government, club or company—should not give us some information about, or some insight into, the nature of the concept of club, government or company. There is this much truth in the objectivist position; Professor Hayek does not and need not deny this.

The point is that Professor Hayek's attempt to restore the unity of idea and object is not possible with a cartesian psychology which isolates men's ideas in individual brains, which divides the world of experience into two: an inner and an outer, and which really cannot account for the fact that individual men in co-operation produce wholes or systems which are non-contradictory. A psychology hampered with the dualism of a *res cogitans* and a *res extensa*, a thinking brain, and private and particular ideas, cannot deal with a social situation.

IV

In fact, Professor Hayek's argument goes deeper than the superficial cartesianism of his description of method and the more real but still unfortunate and irrelevant cartesianism of his psychological terminology. The characteristic of his argument is that he re-introduces the idea or " the way people think " as a constituent part of the social phenomenon which is to be the subject-matter for the social studies. The significance of his argument is that he offers analyses of social phenomena and indicates the implications of these phenomena which are the same, in some cases almost verbally, as those of the school of philosophy which we may call modern Aristotelianism or objective idealism. Even a necessarily cursory examination of Professor Hayek's views of mind and society, of undesigned purpose, of freedom, of totalitarianism and mathematical expression, of individualism, of the historic as compared to the generic account of a phenomenon, and of other concepts with those of Bernand Bosanquet and F. H. Bradley will make this very clear. There is no reason to believe that Professor Hayek is not aware of this; but his use of cartesian psychology may hide the full and far-reaching implications which Aristotelianism has for theories of method of science from the reader, especially from the contemporary non-philosophical reader whose mind is necessarily permeated by cartesianism.

It will be remembered that Bosanquet made a distinction—but not a separation—between the private and personal reason or will of people, and what he called the " real will ", which latter comes into action when two or more people do things together. Then each private reason is transfused by the co-operation so that much more is actually achieved than each one originally intended: a larger reason, so to say, is realised than existed in the minds of each person. The core of Bosanquet's teaching is that social institutions are the reflection of mind, and he says that " mind and society are really the same fabric regarded from different points of view ".[1] In this way he is able to overcome the opposition between government and the individual with which the cartesian empiricism of Hobbes and Bentham fuddled the mind of Mill. Now when Professor Hayek points out that " Not only man's actions towards external objects but also all the relations between men and all the social institutions can be understood only in terms of what men think about them. Society as we know it is, as it were, built up from the concepts and ideas held by the people; and social phenomena can be recognised by us and have meaning to us only as they are reflected in the minds of men ",[2] the student is inevitably reminded of Bernard Bosanquet who wrote that " every social group or institution is the aspect in space and time of a set of corresponding mental systems in individual minds ".[3] Both Professor Hayek and contemporary Aristotelian idealists admit not only the fact of mind in social phenomena but see the origin, in the sense of the generic origin, of social phenomena in mind. The apparent difference between Bosanquet and Professor Hayek on this point comes from the fact, I think, that Professor Hayek is not quite free from scientistic habits of terminology. This appears, for example, in the failure on Professor Hayek's part to make a distinction between the " growth of mind " (whatever that may mean) and the expression " realisation of mind " (which is what mind really does) in the following passage by him: " It is because the growth of the human mind presents in its most general form the common problem of all the social sciences that it is here that minds most sharply divide . . ."[4]—the idea of a growing mind being of course essentially scientistic. Professor Hayek could indeed have been the originator of Bosanquet's statement: " We are about to analyse the modern state into groups of facts which are also ways of thinking ";[1] he would agree " that the science of man must also be the science of mind seems no longer disputable ",[2] and he does say, almost in so many words, " every ' source ' of sociological science is at once a category, or point of view, and also a group of actual social conditions ".[3] As he writes himself, to recognise the existence of mind is to interpret phenomena in the light of our own mind and to admit that they fit into the pattern of our own thinking.

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[1] *The Philosophical Theory of the State* (3rd ed.), p. 157.

[2] IX, No. 35, p. 283.

[3] *The Philosophical Theory of the State* (3rd ed.), p. 160.

[4] XI, No. 41, p. 32.

It is inevitable that, with this mind-permeated social phenomenon as premise, Professor Hayek should make place for a theory of undesigned purpose in society, in common with the Aristotelians. Results, he says, are achieved in the course of social history which have not been aimed at, so that it is clear that social processes advance beyond the aims of a single man and also beyond the combined aims of all men, and he has seen the significance of this fact, for it means that society cannot be engineered or planned. A somewhat scientistic statement of Mr. Schlick is quoted in the course of the argument: " A general principle which proves frequently valid in psychology, and biology and elsewhere: namely that the result of unconscious or instinctive processes is frequently exactly the same as would have risen from rational calculation ".[4] But it is clearly not possible to go very far in the analysis of this fundamental social phenomenon in scientistic terms; philosophers who employ a psychology of mind which is not scientistic know that the phenomenon of undesigned purpose has nothing to do with instinct or the unconscious as such or with anything that can be studied by psychology, biology or physiology. Bosanquet's theory of the " real " will in distinction to the " actual " will is, of course, the attempt to give an explanation of the fact that man's actions go beyond himself and his individual reason. " We live in our objects," writes Bosanquet,[5] " but we do not know how or how far our objects identify us with the wholes to which we ultimately belong ". His teaching is that it is to the extent that we in fact in social action identify ourselves with things beyond our isolated plans that we realise ourselves and social progress, in this way bringing to completion a larger whole which is much beyond ourselves and which we know only partially, from our own standpoint. The fact that our actions are largely what we may call short-doctrine actions and do not involve deep theories of what is right and what we ought to aim at for the far-distant future, and yet lead to achievements greater than their intentions, is, of course, an integral part of F. H. Bradley's philosophy. " The individual in his individual work thus accomplishes a universal work ", he writes, and he does this not by profound theories about reality on which he bases planned actions, for the non-theoretical person if he be not immoral is at peace with reality.[6] Bernard Bosanquet makes the same point when he writes: " For us, then, the ultimate end of Society and the State as of the individual is the realisation of the best life. The difficulty of defining the best life does not trouble us, because we rely throughout on the fundamental logic of human nature *qua* rational ".[7] And we are led to look for a clue to our real will " in the social spirit as incorporated in laws and constitutions, that is to say, in society as a working whole reflected in the full consciousness of the system which composes it ". If we substitute Professor Hayek's unconscious purpose for the real will, and his coherent wholes or structures for social spirit his sentence might have come out of ECONOMICA. The fact of undesigned purpose is seen on analysis to imply a rational human nature. It is this type of mind—and not that of cartesian mental atoms—which is involved in Professor Hayek's description of a social situation: " In the social sphere these spontaneous movements which preserve a certain structural connection between the parts are, moreover, connected in a special way with our individual purposes: the social wholes which are thus maintained are the condition of the achievement of many things we individually aim at, the environment which makes it possible even to conceive most of our individual desires and which gives us the power to achieve them ".[8]

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[1] Bosanquet, *loc. cit.*, p. 237.

[2] *Ibid.*, p. 42.

[3] *Ibid.*, p. 44.

[4] XI, No. 41, p. 28.

[5] Bosanquet, *loc. cit.*, p. 163.

[6] *Ethical Studies*, Essay V, " My Station and its Duties ", esp. pp. 183–206. The analysis of the relation between individual and social whole which this essay contains should be studied by every social scientist, for its psychology is very sound and enlightening. It should be noticed that in this essay the analysis of " social wholes " starts from the individual aspect of them, and that—in the next—the individual is seen to develop beyond the state, for " Man is not man at all unless social, but man is not much above the beasts unless more than social " (p. 223).

[7] Bosanquet, *ibid.*, p. 169.

[8] XI, No. 41, p. 28.

One point should be noticed here. Through a process of scholarship which is very difficult to follow, this view of the nature of mind and its place in the social whole has been associated with some form of political totalitarian theories. But it should be remembered that Bradley did not stop writing philosophical essays when he had written "My Station and its Duties"; he wrote an equally important essay entitled "Ideal Morality", in which he clearly points out the limitations of the action of the state on the life of the individual and how the individual goes beyond. It is also difficult to see how this passage by Bosanquet could have been ignored: "Neither the State, however, nor the idea of humanity, nor the interests of mankind, are the last word of theory . . . the human mind, consolidated and sustained by society, goes further on its path in removing contradictions and shaping its world and itself into unity. Art, philosophy and religion, though in a sense the very life-blood of society, are not and could not be directly fashioned to meet the needs and uses of the multitude, and their aim is not *in that sense* "social". They should rather be regarded as a continuation within and founded upon the commonwealth, of the work the commonwealth begins in realising human nature; as fuller utterances of the same universal self . . ."[1] Professor Hayek is saying the same thing from a different angle and with a different problem in mind when he writes: "What all the people who are so unwilling to renounce any of the powers of conscious control seem to be unable to comprehend is that this renunciation of conscious power, power which must always be power by men over men, is for society as a whole only an apparent resignation, a self-denial individuals are called upon to exercise in order to increase the powers of the race, to release the knowledge and the energies of the countless individuals that could never be utilised in a society consciously directed from the top. The great misfortune of our generation is that the direction which has been given to its interests by the amazing progress of the natural sciences is not one which assists us in comprehending the larger process of which as individuals we merely form a part, and in appreciating how we constantly contribute to a common effort without either directing it or submitting to orders of others. To see this requires a kind of intellectual effort different in character from that necessary for the control of material things, an effort in which the traditional education in the "humanities" gave at least some practice, but for which the now predominant types of education seem less and less to prepare".[2] Professor Hayek is not far from the position of Bosanquet who writes: "Individuals are limited and isolated in many ways. But their true individuality does not lie in their isolation, but in that distinctive act or service by which they pass into unique constructions of the universal".[3] He has himself described the phenomenon noticeable in the society that when individuals co-operate something more is achieved than the sum of their parts; and the idea that this "passing into the universal" is a conscious process is fallacious: we have already noticed Bosanquet's apt phrase about our trust in the internal logic of human nature *qua* rational.

That Professor Hayek's psychology must be only apparently cartesian and that its scientism must be only on the surface is, of course, clear from the place which he ascribes to "the way people think" in social phenomena, and from the characteristics which he ascribes to reason, as we have seen. His scientistic terminology does permit Professor Hayek to say that what are facts for oneself can also be facts for other people and to speak of the "similarity" of minds. It does not allow him to proceed further to ask what the implications of these observed data, common facts and similarity of minds, may be. Bernard Bosanquet has made a few brief but illuminating remarks on this point.[4] He points out what in our day has become an obvious fact, that the positivist attempt to study social phenomena has by the sheer logic of facts brought investigators to psychology in the study of social phenomena so that it seems no longer disputable (almost using words of Professor Hayek) "that a science of man must be a science of mind". But then Bosanquet's argument for the necessity of logical criticism in contemporaneous psychological treatment—what is in effect a criticism of psychological scientism—follows immediately. Because of its roots in physical analogy, contemporary social psychology is apt to regard as separate what really belongs together, so that the difference and the identity which are found in any whole or self-contained structure are regarded as if they belong to different realms, "and the *one* and the *other* are regarded as reciprocally exclusive atoms". The conception of a oneness holding together differences is not available on scientistic thinking and it is clear that, if Professor Hayek had permitted himself a more profound psychology, he would have proceeded further in his analysis of the subject-matter of the social sciences. This is implied in Professor Hayek's own statements. To take a further example, he writes: " If the social structure can remain the same although different individuals succeed each other at particular points, this is not because the individuals which succeed each other are completely identical, but because they succeed each other in particular relations, in particular attitudes they take towards other people and as the objects of particular views held by other people about them. The individuals are merely the *foci* in the network of relationships and it is the various attitudes of the individuals towards each other (or the similar or different attitudes towards physical objects) which form the recurrent, recognisable and familiar elements of the structure. If one policeman succeeds another at a particular post, this does not mean that the new man will in all respects be identical with his predecessor, but merely that he succeeds him in certain attitudes towards his fellow man and as object of certain attitudes of his fellow men which are relevant to his function as policeman ".[1] What Professor Hayek here refers to as " attitudes of the individuals towards each other " are really, of course, the ideas that people have about the function of the policeman; and these ideas cannot be atomistic and separate, nor even merely similar, if the police system is to function. They must have the quality of non-contradictoriness, or universality, if the system is to work. It is clear that a scientistic psychology cannot control this social situation, and that Professor Hayek has gone much beyond it.

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[1] Bosanquet, *ibid.*, p. 309 f.

[2] XI, No. 41, p. 38.

[3] *Ibid.*, p. 169.

[4] *Ibid.*, ch. 11.

This is again carried out by Professor Hayek's treatment of the historic method and of the historic nature of social phenomena. It is clear that on a cartesian psychology and method the true account of a social fact will be the historical account, but that on a pre-cartesian or realistic theory of ideas this will not be the case. Professor Hayek leaves no doubt in the minds of his readers as to the limitations of historic method and treatment when applied to social phenomena. In fact, he reverts to Aristotle's position when he points out that an " historical " account of social phenomena is more often than not only apparently historical. The statement that we are really concerned " with a dependence of the parts of the phenomenon upon each other which is not even necessarily the same as the chronological order in which they appear "[2] brings us to Aristotle's analysis of method in the first book of the *Politics* when he says that " He who considers things in their first growth and origin, whether a state or anything else, will obtain the clearest view of them " but goes on to say that " the state is by nature clearly prior to the family and the individual since the whole is of necessity prior to the part ". Professor Hayek writes: " Though the explanation of the way in which the parts of the social whole depend upon each other will often take the form of a genetic account, this will be at most ' schematic history ' which the true historian will rightly refuse to recognise as real history. It will deal, not with the particular circumstances of an individual process, but only with those steps which are essential to produce a particular result, with a process which, at least in principle, may be repeated elsewhere or at different times. As all explanations it must run in generic terms, it will deal with what is sometimes called the logic of events, neglect much that is important in the unique historical instance, and be concerned with a dependence of the parts of the phenomenon upon each other which is not even necessarily the same as the chronological order in which they appeared. In short, it is not history, but compositive social theory ".[3]

Indeed, to conclude this cursory examination of the contemporary Aristotelian elements in Professor Hayek's philosophy, his description of the constructs or wholes in society which are the familiar subject-matter of the social sciences, with its emphasis on their individuality, their self-contained nature and their unhistoricity might have come straight from Bosanquet. When Professor Hayek pours scorn on those people who attempt to explain social phenomena from the outside we cannot but be reminded of Bosanquet's striking opening statement in the first chapter of his *Philosophic Theory of the State* in which he discusses scientism and philosophic method. ". . . a philosophic treatment is the study of something as a whole and for its own sake . . . It deals, that is, with the total and unbroken effect of its object. It desires to ascertain what a thing is, what is its full characteristic and being, its achievement in the general act of the world. History, explanation, analysis into cause and conditions, have value for it only in so far as they contribute to the intelligent estimation of the fullest nature and capabilities of the real individual whole which is under investigation ".[1]

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[1] IX, No. 35, p. 284.

[2] XI, No. 41, p. 30.

[3] *Loc. cit.*

We have, of course, not by any means exhausted the examples of idealistic implications in Professor Hayek's analysis of the method of the social sciences. The tenor of his treatment of statistics and numerical description is in line with the short mention of the same subject by Bosanquet, and Bosanquet's problem—and Rousseau's—of the legislator who must be better than himself is, of course, an aspect of the problem of unconscious design. So also Professor Hayek's discussion of the influence of nineteenth century scientific concepts on social science in the first and third sections of his articles should be read with chapter two of Bosanquet's *Philosophic Theory of the State*. What we have ventured to call Aristotelian or objective idealism—perhaps we should have used the expression realist idealism, for, as Bosanquet pointed out, the difference between idealism and realism seems to disappear when things are thought out—does not imply the full, omniscient consciousness which is necessary for planned society. Nor does it imply a planned society as ultimate in history in its theory of the increasing manifestation of reason in things. The fundamental principle of Aristotle's social philosophy—that of whole-and-part—clearly underlies Professor Hayek's analysis of the nature of social phenomena, as he also makes place for the individuality both of humans and of social wholes.

V

If all this is so, several troublesome problems must occur for the theory of subject-matter of economics and of method of economics.[2] (*a*) The first one I will call " the problem of the double premise ". Professor Hayek has himself pointed out that the subject-matter of the social sciences involves the ideas in people's minds about the phenomena under observation. The subject-matter, so to say, consists of the object-out-there as well as of the ideas in the minds of the people. Therein is the difference between it and the subject-matter of the physical sciences. But scientific method while it was still nearer to its cartesian origin and not extended to psychological and social phenomena assumed a premise commonly called the uniformity of nature and on this basis it was able to establish its predictions. One finds the idea of uniformity or system expressed or implied in the work of most social theorists and the implications of it have left their footprints all over the theories. The conception of a beneficent Author or chief engineer of " the obvious and simple system of natural liberty "—somewhat like a Leibnitzian pre-established harmony—is assumed by Adam Smith, and Mr. Cliffe Leslie has pointed out to what extent the assumption has affected his views. Professor Robbins writes that " the unity of subject-matter of Economic Science " is " the forms assumed by human behaviour in disposing of scarce means ", obviously expecting that the behaviours will show some kind of uniformity, otherwise there could not be a unity for the data to be scientifically or even methodically treated. If, however, as Professor Hayek suggests, the subject-matter of the social sciences should also involve the ideas in people's minds about the phenonemon then a second premise has to be introduced, and that is that also ideas (and actions resulting from ideas) involve the principle of the uniformity of nature. It is not enough to make the difficult distinction between constitutive ideas and ideas about constitutive ideas, and to point out that the constitutive idea is more stable and fixed, so to say, while ideas about it are more changeable and so apparently less susceptible to methodical treatment. The constitutive idea, as part of the subject-matter of the social phenomenon, must be describable in terms of some kind of categories comparable to those involved in the uniformity of nature premise, if there is to be a science of economics or of social phenomena.

It may be argued by some people that the distinction we have indicated between the two aspects of the social phenomenon in working out the implications of Professor Hayek's analysis—that is, between the constitutive-idea-aspect of it and the external or behaviour-aspect of it has been made too sharp and so unreal by us. It should be remembered, however, that people often help to create a social phenomenon, for example, a monopoly, and to maintain it, without knowing what they are doing, so that it appears that the constitutive-idea side may differ sufficiently from the external aspect to justify a clear distinction and to raise the problem of the double premise.

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[1] p. 1.

[2] In this connection the article by R. G. (later Professor) Collingwood, " Economics as a Philosophical Science " in *The International Journal of Ethics*, Vol. 36 (1925), should be read.

(*b*) But even if the problem is not admitted as stated in the form of the double premise and some kind of close unity of idea-aspect and behaviour-aspect in the social phenomenon is postulated, the problem is not really avoided. Scientific method taken in any sense which gives any kind of meaning to the words involves the use of hypothesis and analogy in its processes. These two closely allied procedures have to do with the seeing of relations between phenomena and it is exactly here that the intelligence and the perspicacity of the scientist must appear. Sometimes the relations of similarity which link the phenomena in the mind of the investigator are superficial, sometimes closer to the nature of the thing and pointing to a real underlying unity or identity. Analogy involves the feeling for or suspicion of similarity between the two sets of terms; hypothesis goes further and involves the hope or expectancy of an identity. Adam Smith uses at least two sets of analogy-hypothesis arguments: the first that the system of economic relations is the same as the system of natural relations, and the second that the subject-matter of economics is of such a kind that the empirical methods of the natural sciences can be applied also in the study of economic phenomena. List speaks of social *forces*; Bastiat's " great, admirable, comforting, necessary and flexible law of capital " with its confusion of ethical and physical analogy is well known. It may, of course, be pointed out that Aristotle also used analogy in his study of the social sciences for he constantly refers to principles governing natural phenomena when he is describing social phenomena. But the difference between him and the moderns is that he justified his use of analogy by the Greek principle of the rational nature of the unity of nature which was discovered when it was discovered that law *physei* or by nature was the same as *nomos* or rational law. Constantly recurring statements in the writings of social scientists, such as that " social phenomena are invariable and of known frequency and that this gives us social laws (when the question is not asked to what extent the phenomena are still ' social ' if they exhibit these characteristics), and the use of terms such as mental forces, fields, momentum, energy, economic dynamics, equilibrium, statics, measurement and evolution show only too clearly the acceptance of the similarities and identities involved in the use of physicist, biological and historical analogy and hypothesis ".

But to the extent that the constitutive idea becomes part of the social phenomenon these hypotheses will not be usable, unless we are prepared to accept a mental chemistry psychology which out-atomises the atomists. And if we cannot use scientific similarities and identities in our research into social phenomena—or at least must use these very sparingly—what remains? It seems that the only thing that remains is the method of Aristotle, which is also called philosophical analysis.

(*c*) The introduction in any sort of way whatever of the constitutive-idea-aspect into social phenomena raises the question of the nature of the subject-matter of the social sciences, as Professor Hayek very rightly points out. More particularly it raises the question of the nature or distinguishing marks of the various social sciences, for example, as to when a phenomenon, or an aspect of it, is economic or political or " sociological ", etc. For ideas are not compartmentalised and the human mind is part of all these classes at one and the same time. For example, the concept of " scarcity " hardly offers a sufficient common basis for distinguishing economic acts from other acts, for the idea of scarcity which leads to the behaviour may be the outcome of an urge, desire, want or motive and there is no reason to believe that the act arising from one of these as constitutive idea need necessarily be " economic " or " social ". Nor does scarcity as constitutive idea in any of these forms indicate under which heading the particular phenomenon or aspect of it is to be treated. The chances are that actions resulting from wants may be economic in their nature more truly than those resulting from a constitutive idea having the nature of a motive, for it has been well said that man's real aims are not the satisfaction of want but making greater wants of a certain type called good—which is what Aristotle said was the end of political society. It is clear that when the idea-aspect is retained as constituent part of the social phenomenon—as it clearly must be—distinctions between social sciences become purely artificial. In fact also no social phenomenon is purely economic, political or sociological. Indeed, it becomes difficult to distinguish between individual and social phenomena. The idea occurs that the basis on which we shall have to distinguish between the various social sciences will not be the appearance or the results of the action or organisation, but the purpose contained in the constituent idea; thus an apparently economic phenomenon may really have to be classified as political. But then the social sciences would become teleological. For when the constitutive idea becomes part of the phenomenon what in fact happens is that the significance of the phenomenon is admitted as part of the object of study—and the phenomenon is not studied merely scientistically.

The problem of scientism in the social sciences is not exhausted when it is stated as a criticism of the externalising and objectifying of social phenomena in the form of objectivism, collectivism and historism. It goes further. It involves, and particularly involves, the question of the categories to be applied in the study of this type of phenomena. Plato and Aristotle, who did not commit the errors of scientism and did not lose sight of mind in social phenomena, discussed the phenomena of economics, politics and social psychology, the one with the illumination of æsthetic analogy in terms of balance, wholeness, harmony, individuality, the other with ethical analogy, in terms of true function, whole-and-part, the just limit. Professor Hayek has shown clearly the limitations of the use of measurement and statistics in the study of social phenomena and we have tried to show that the same line of argument can be applied to the application of any of the scientist's categories to the subject-matter of the social sciences. The distortions which concepts such as " individual ", " value ", etc., undergo when they are used by scientistic social scientists indicate this very clearly.

The significance of Professor Hayek's timely articles goes beyond the remarkable insight they display into the subject-matter of the social sciences and the method which is thereby involved—for which alone they are extremely important. The articles are significant for the clear indication they offer of the philosophical principles revealed, by the sheer logic of facts, in the studies of social phenomena during the last three-quarters of a century—a philosophy which we have called, for want of a better term, Aristotelian or objective idealism. At least four conclusions appear to follow from Professor Hayek's analysis of the subject-matter and the method of the social sciences: (*a*) that the categories of the natural sciences are untenable in the treatment of social phenomena; (*b*) that cartesian or " modern " psychology is of no (or very little) use in the study of social phenomena; (*c*) that the term science is stretched into insignificance when it is applied to the study of social phenomena, and since the study of social phenomena clearly involves the study of things of the mind what are generally known as social studies are in fact philosophy; and (*d*) that philosophers should now extend their critical and analytical activities also to include the proceedings and thinking in the social studies.

Capital Requirements in Progressive Economies

By Ernest H. Stern

In this article an early and still crude attempt is made of examining the correlations which over long and medium periods exist between a particular form of spending, viz., spending on capital goods, and the growth of the social income.[1] With all the theoretical reservations in respect of projecting into the future average relationships prevailing in specified periods and with all the practical confidence resulting from the disclosure in recent decades of the continuity of economic trends over long periods, these correlations may be made the basis of estimates of long-term average requirements for the division of the annual social income between spending on comfort and on capital. Deductions may also be made with regard to the level of interest, the fluctuations of economic expansion and related problems.

As the statistics available at present vary in precision, the conclusions must be regarded as mere approximations which may be gradually improved as more facts come to light; they are presented before the statistics have been perfected largely to stimulate further research. Imperfect as they are, they provide us with a measure of the way in which in the past average capital expenditure or the growth of capital employed, has been correlated with the expansion of the National Income. Only the correlation between the two factors is disclosed by the statistics. We know, however, that output is a function, *inter alia*, of equipment, i.e., of capital employed. As the ratios prove to be persistent over long periods in the case of the U.S., and fairly persistent over medium terms in the U.K. and the Union of South Africa, they may be regarded as an approximate indication of the capital *requirements* on which the expansion of the National Income (and of general welfare) depended during the periods under review. Taking account of the continuity found in respect of the trends of economic expansion and many other economic correlations (which, as far as our present knowledge goes, extra-economic events—even such catastrophies as war or revolution—have no more than temporarily distorted) there is reason to expect that after the present war these trends will re-appear. In this case, the ratios deduced from the examination of the past will be a guide to the order of magnitude of the capital requirements with which the various countries will be faced once they have repaired the damage wrought by the war.

Capital is understood to include producers' man-made durable goods, all buildings and construction, and in the case of the U.S., business inventories; (it would have been more correct to include business inventories in all cases, but available statistics do not allow this.) It does not include land, other than its improvements, nor consumers' durable and semi-durable goods.

The correlation that exists between the amount of capital employed and Net National Income is illustrated by the following scatter diagrams for three countries, the U.S.A., the U.K. and the Union of South Africa. The diagrams are based on statistics of different quality. The figures for the U.S. (S. Kuznets) are the most complete and most reliable and cover both pre-1914 and post-1914 periods over 50 years. They will form the main basis of the argument. Those for the two other countries cover 15 and 17 erratic years of the inter-war period. In their case, moreover, adjustments for price fluctuations and, in respect of the U.K., for obsolescence are of the crudest kind. Lastly, diagram III is based not on capital employed in the individual years, for which statistics are not available, but on the cumulative net capital investment, i.e., on the capital employed in the individual years, minus a constant representing the capital employed at the beginning of the period. The ratio of regression is not more than a rough average, and this diagram is used merely as corroborating evidence.[1]

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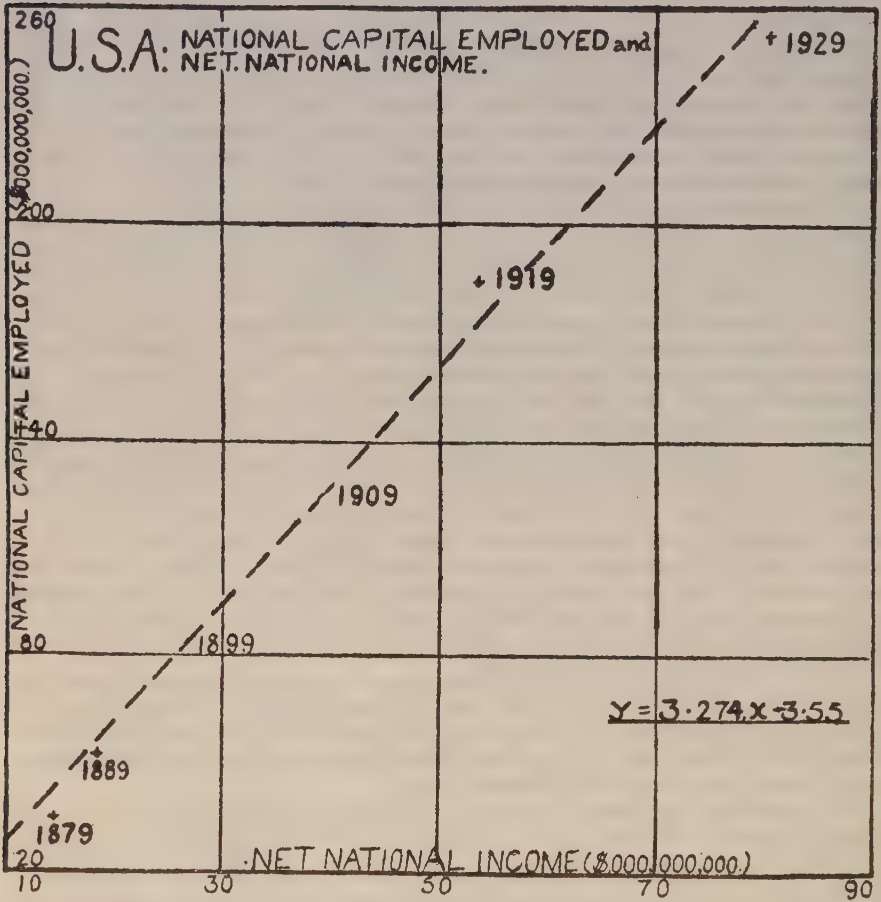
[1] For several years Carl Snyder has directed attention to, and provided evidence of, the growth of capital employed and of output in various industries and the whole manufacturing industry of the U.S. proceeding at equal or nearly equal rates over long periods. He thereby anticipated and confirmed some of the findings of this study. The relation to the later of Snyder's investigations will be explained further down in footnote (1), to page 169. The relevant studies of Carl Snyder are his articles on "Die Bedeutung des Kapitalangebotes fur den Industrialisierungsprozess", *Weltwirtschaftliches Archiv*, 1935; "Capital Supply and National Wellbeing", *American Economic Review*, 1936; and his book *Capitalism the Creator*, New York, 1940.

The figures for the U.S. include in the account of income and capital the balances resulting from international investments, but those for the three other countries have been crudely corrected so as to include only home-produced income (termed " output ") and capital expended at home.

1. U.S.A. 1878 to 1928. The data used are those of S. Kuznets.[2] The income figures are annual averages of the decades ending five years after the dates for capital employed. They do not represent the actual income at the particular date for which the capital employed has been counted, but rather the net income capacity at that date at annual rates, i.e., an income capacity at an average use of equipment. They illustrate the *average level* of income in annual rates at the end of decades. The figure for 1928 is an estimate by the present writer of income at a presumed average use of equipment, based on the average of the actual incomes of 1928/29, and the figure for 1878 a similar estimate based on the averages of the two overlapping decades 1878/88 and 1884/93. All figures are in values at prices of 1929.

DIAGRAM I



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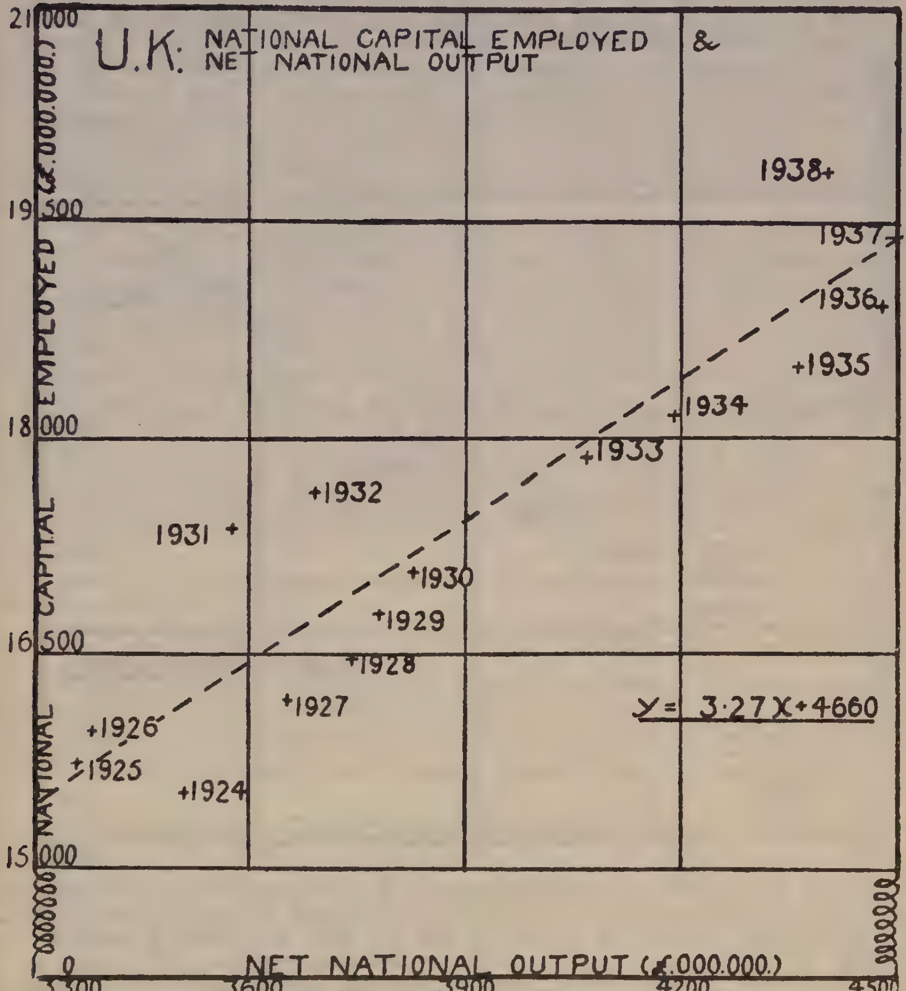
[1] The results in respect of Germany are subject to so many reservations that they are not used even as corroborating evidence. As they disclose, however, many interesting features from the point of view of this investigation, they are given in footnotes.

[2] Most figures are taken from *Uses of National Income in Peace and War*, Occasional Paper No. 6 of the National Bureau of Economic Research, N. York, 1942. Capital employed taken from " Capital Formation 1872—1938 " (Address to the University of Pennsylvania, Bicentennial Conference, p. 67).

The correlation is very close. The slight diversions around 1918 and 1928 are partly the by-products of the World War I. The rate of regression indicates that in the 50 years under review an increase of capital employed, i.e., a net capital investment (including inventories) of about \$320 to \$330 (\$327 as calculated) was associated on average with an increase of \$100 in the level of the Net National Income.

2. U.K. 1924 to 1938. The figures for the Net National Income are those of Professor A. L. Bowley for 1924 to 1937,[1] linked to the 1938 figure of the White Paper Cmd. 6520/44. This series has been adjusted so as to exclude the balance of income from abroad and to convert current values into those at prices of 1938.[2] Being restricted to home-produced Net National Income, the series is called Net National Output. The figures for the capital employed have been arrived at as follows:—From the Campion estimate of public and private property in Great Britain for 1932–34[3] in the form given by Hicks[4] there have been selected the assets representing producers' durable goods and all construction. This total excludes the

DIAGRAM II



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[1] Taken from his address given before the Royal Stat. Society, 1940 (*J.R.S.S.*, 1940, p. 517).

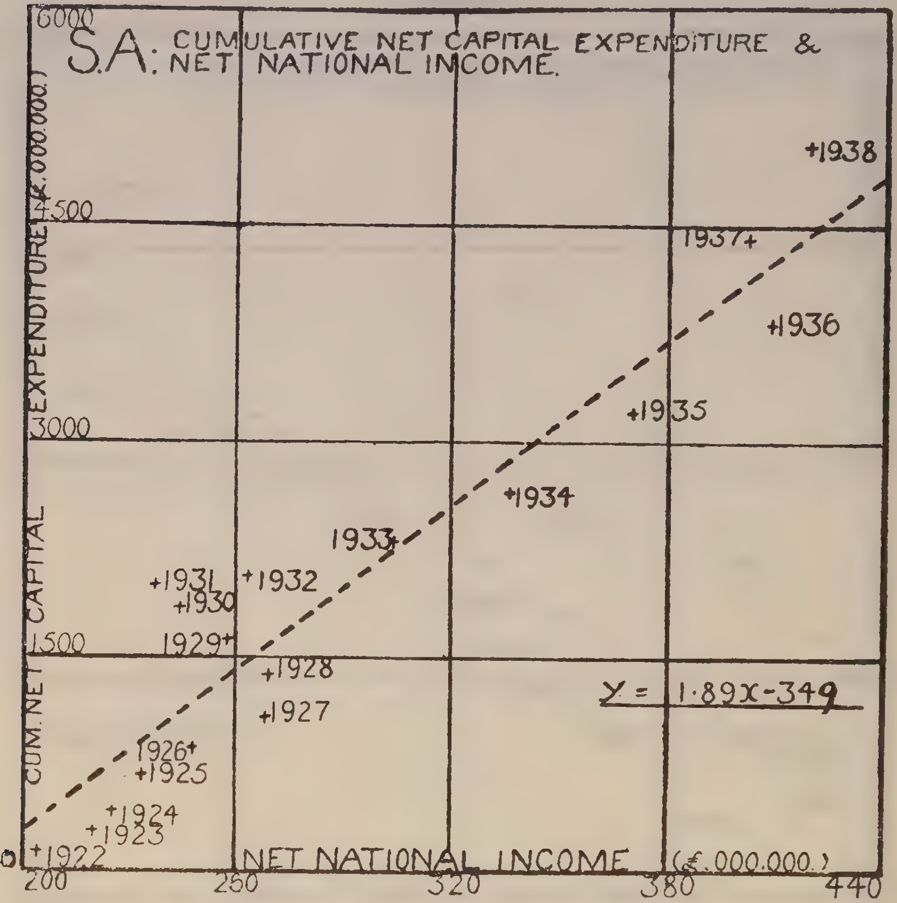
[2] The latter with the help of an index, being the average of the indices of the Cost of Living and Manuf. Goods prices, the latter estimated by this writer for the years 1925/29.

[3] H. Campion, *Public and Private Property*, Oxford University Press, 1939.

[4] J. R. Hicks, *The Social Framework*, Clarendon Press, Oxford, 1942.

value of the road system, but includes the value of land, agricultural as well as built-up. It also excludes the value of armaments, of gold and the balance of foreign investments. We assumed the balance of the values of land and roads to be small enough to use that figure (of £16,250 Millions) as an approximation to a total of producers' man-made durable goods, all buildings, construction, business inventory and the improvements to the land. This magnitude has been assumed to represent the capital employed at the end of 1933. It has been converted to its value at prices of 1938 by the price index for capital goods used in converting the annual capital expenditure to values at prices of 1938 and described further down in footnote 1 to p. 167. By adding to it the cumulative net annual capital expenditures for each year from 1934

DIAGRAM III



to 1938 (at 1938 prices) and deducting from it the cumulative net annual capital expenditures for each year from 1923 to 1934 (at 1938 prices) a series of annual totals of national capital employed at the end of each year 1923/38 or, what is practically the same, at the beginning of each year 1924/39 has been obtained. This series of values of capital employed at the beginning of years has been correlated to the values of income for the respective years. As the basic Campion figures include business inventories, but the annual net capital expenditure excludes them, the series of capital employed presumes that business inventories, adjusted for price fluctuations, have remained at the 1932–34 level throughout the 1924–38 period. This assumption is probably incorrect, but the error involved probably small. The gross national capital expenditure at home has been taken from Bretherton, Burchardt, and Rutherford: *Public Investment and the Trade Cycle* (Oxford 1941) for 1924/37[1] and from the White Paper Cmd. 6520/44 for 1938, converted into values at prices of

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[1] P. 402, Table 4, Col. 1.

1938[1] and adjusted to net expenditure by a crude estimate of normal wear and tear, other than for 1938, when net investment has been taken from the White Paper. It excludes armament expenditure and inventories. The net investment would have been smaller and the series more correct in the period under review if in respect of residential buildings not an assumed " normal " rate of depreciation had been deducted, but a rate taking regard of the replacement of slum houses. The National Income would have been larger if the money equivalent of the satisfaction derived from using the roads for private motoring had been included, say the income that would otherwise have accrued to transport companies.

The result of the calculation is that on the average a net capital expenditure at home (excluding inventories) of £327 corresponded to each £100 growth of Net National Output.

3. South Africa, 1929 to 1938. The statistics of the Net National Output and of the net capital are those of Professor S. Herbert Fraenkel.[2] Net capital expenditure excludes inventories but includes the balance of imported capital; the Net National Output is the income before deduction of the net balance of interest and dividends paid to non-residents. The comparison, therefore, is between the increments of capital employed (excluding inventories), irrespective of who provided this capital, and the output produced from the capital employed, a comparison which appears appropriate in the case of a capital-importing country in the course of development. The current values in Professor Fraenkel's estimates have been reduced to values at prices of 1929.[3]

The ratio of capital expenditure to growth of output is only £189 to £100 and is thus substantially smaller than in U.S.A. and U.K. Further investigations are needed to show whether this comparatively low ratio in fact prevails or whether our result is due to faulty statistics, and, if the result is correct, whether it is significant for the period under review only. In this period windfall income may have been included in so far as the very substantial fall of commodity prices in terms of gold may well have increased the profitability of all capital, including the capital employed at the beginning of the period, considering the great proportion of income in S.A. resulting directly and indirectly from gold mining. Another contributory factor may be found in the exclusion of a depletion charge from the National Income. If such charges were debited in all three countries, that of S.A. would presumably be the proportionately highest and the reduction of net income the largest. A further reason may be the erratic nature of the period under review and its effect on calculated averages.[4]

[1] With the help of an average of the indices of Building Costs and Manufactured Goods prices: 1925/29 estimated by this writer.

[2] Revised figures from his Memo., " An Analysis of the Growth of the National Income, etc.", 1944.

[3] The National Income figure by way of the " All Retail " price index and the Capital Expenditure by way of the price index of " Manufactured Goods ".

[4] Germany: 1925–37. The statistics of Net National Output are those of the Central Statistical Office of Germany for Net National Income before payments on Reparations, in terms of purchasing power of 1928 (*Statistical Abstracts for Germany*, 1933, 1935, and 1938). They have been roughly adjusted by this writer to represent the output before any balance of interest and dividend paid to Non-Residents. (On the basis of official estimates of the " Balance of Payments ", given in the *Statistical Abstracts for Germany*.)

In the case of Germany one is faced with the difficulty of the particularly severe decline of activity in the early thirties and the partial statistical blackout after 1934. Moreover, fluctuations in inventories were extraordinarily large and these fluctuations were more than in other countries connected with international capital movements. In Diagram IV, therefore, the four years of depression, 1930 to 1933, have been omitted when calculating the line of regression and, in consequence, that average is based on only nine not even fully consecutive and moreover rather hectic years. It is subject to a large margin of error.

The correlation of the nine years, excluding 1930–33, is not very close, but correlation there still appears to exist. The calculated ratio is 1.92 indicating a Net Capital expenditure of R.M. 192 to be associated with each R.M. 100 change of Net Output. This ratio is low as compared with that in the U.S. and U.K., two countries of an economic structure on average not very dissimilar to that of Germany in the period under review. One reason may be the error resulting from calculating an average from such scanty material, as explained above. Another weighty reason may be the probability that the official calculation of Net Capital expenditure of Germany is a *substantial under-statement* as compared with the estimates for the two other countries, deductions for " normal " wear and tear having been at a much higher rate. These deductions in the German official estimates are as high as ⅔ of gross, whereas in the other Countries they are about ½ of gross. On the basis of equal methods of accounting the ratio in Germany of Net Capital expenditure in relation to net increase of output would be close to 3, including such errors as may be due to the unsatisfactory sample.

In the case of Germany some kind of test is possible by relating the estimate of Helfferich of the National Wealth in 1913 (*Deutschland's Volkswohlstand*, 7th Edition, 1917), to the Net National Output

The diagram in respect of the U.S., on p. 164, suggests a constant ratio and, as it is based on decade values, the time-lag factor has been largely covered. In respect of the diagrams for the U.K. and S.A. on pp. 165 and 166, doubts are permissible whether a rising ratio and the correlation of current capital to net output of a slightly later period would not fit better. In view, however, of the short and erratic periods involved in the latter case, the precariousness of converting value statistics for such periods to values at stable prices, the comparatively good fit obtained for most years, other than 1930/32, and last, but not least, lack of time in present conditions, this writer abstained from attempting further refinements.

The relations represented by the three diagrams may be stated as coefficients representing the average rate of net capital expenditure accompanying a given growth

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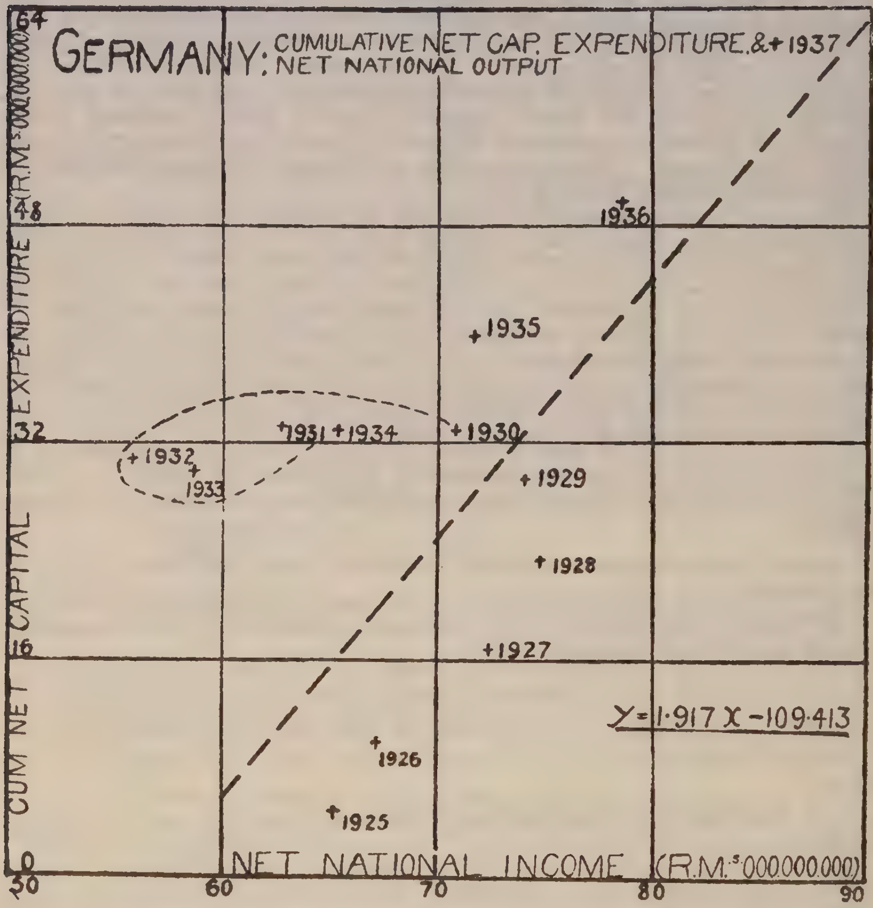
DIAGRAM IV



of that year, adjusting the estimates of National Wealth, so as to exclude the balance of investments abroad, land, and household equipment, to make it represent "Capital" goods and business inventories. Net Output may be derived from the calculations of the Central Stat. Office of the Net National Income for 1913. (*Das Deutsche Volkseinkommen*, etc., "Einzelschr. des Stat. Reichsamtes, No. 24, 1932.) The estimates thus adjusted are as follows:—

		1913
National "Capital" employed	M. Mlds.	173.0
Net National Output		48.8
Ratio of Net "Capital" employed per M. 100 Net National Output	M.	355

This ratio of Capital to Net Output is only slightly higher than the ratio for Capital employed in the U.S., which can be calculated as $322 in 1909 and $340 in 1919 for each $100 Net National Income, the latter including balances of income from foreign investments.

of net income. It would appear that during the period under review the numerical value of this coefficient was somewhere between 2 and 3.5.

II

Though the underlying statistics are not equal in quality and do not cover the same periods, the persistence of a ratio close to 3 to 3½ in two out of the three countries, and of a ratio still as high as close to two in a third country is unlikely to be merely fortuitous. At least it may be regarded as *prima facie* evidence that in conditions such as prevailed in the periods under review the expansion of economic activity, as reflected in net output of goods and services, did depend on additions to the stock of capital at the average rate of say between 2 and 3½ times the expansion of economic activity. If further research were to confirm this first approximation, the way would be prepared for various important conclusions, some of which will be indicated in the following paragraphs.[1]

The coefficient here evolved gives a numerical expression of capital investment per unit of growth of income in progressive contemporary societies. Multiplying it by the *rate* of growth provides a numerical expression of the annual capital requirements as % of the National Income, in other words, the required savings rate.

Economic activity in the U.S. in the period 1878/29 expanded at a slowly diminishing rate which, according to calculations of the Federal Reserve Bank of New York, was 5.5% p.a. in the late 1870's; 4.8% at the turn of the century and 4% in the late 1920's—all at compound interest. The Net National Income, calculated at stable prices appears, however, to have expanded at the fairly stable rate of 3¼% p.a., compound interest, in the same period.[2] Industrial production in the U.K., as expressed in indices of production, which, however, usually suffer from a downward bias, may be estimated to have expanded at the fairly stable but comparatively low rate of 1¼% p.a., compound interest, throughout the 20th Century, the decline in the first World War having been offset by a more than average increase in the late 1920's and in the 1930's. Net National Income may have risen at a similar average rate. In the period 1924/38, the Net National Income at stable prices used in diagram II, shows a fairly stable expansion of 2.3% p.a., compound interest, whereas industrial production, as reflected in indices of production, appears to have risen by 2.5%, compound interest, in the same period. Net National Income in South Africa at stable prices, as used in diagram III, shows an expansion at the high rate of 4½% p.a., compound interest, for the period 1924/38.

The difference between the rates of growth in the three countries is partly due to the different rates of increase of population and partly to different rates of growth of income per head. Let us make various assumptions:—Firstly, in respect of income per head, viz:—

(*a*) The people of U.K. in future will endeavour to keep pace with the U.S. in respect of the progress of Net National Output *per head*, say at a rate rising at 2% p.a. at compound interest.

(*b*) The progress per head in the U.K. will fall behind the U.S. by ½% p.a.

Let us assume too that the population will increase at ¼% p.a. in the U.K. and at 1% in the U.S., compound interest. Assume also that the people of South Africa will endeavour to maintain their pace of progress at a rate of 4½% p.a., their increase in population having been close on 2% in the 1920's and 1930's. Assume lastly that the requirements of accretion to capital employed, i.e., the net capital expenditure, will be 330 both in the U.S. and the U.K. and 250 in South Africa per 100 Units of growth of net output, the increased requirements in S.A. being due to the possible rise of prices of commodities and services in terms of gold. Under these assumptions, the potential long-term capital requirements associated with economic expansion in the three countries (disregarding additional requirements of the U.K. for rehabilitation or war debt payments) can be estimated as follows:—

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[1] In his studies referred to in footnote (1) to page 163 Carl Snyder has restricted himself to showing that over long periods capital employed and output were growing at equal or nearly equal rates. He did not examine, however, *how much* capital was invested in relation to growth of output. Only in respect to the Manufacturing Industry of the U.S. did he express an opinion on this relation in a summary statement as follows (*Capitalism the Creator*, p. 126): " For every dollar of additional value added per annum in manufacturing, as far back at least as 1850, something more than an additional dollar of new capital has apparently been required. This ratio of capital to annual value of product has been rising slowly in the eighty years from 1850 to 1930, so that it now requires nearly twice as much capital to produce a given value of output ". Snyder's evidence, provided by diagrams of growth, is concerned with a number of particular industries, the whole manufacturing industry, and in one instance with the parallelism of growth of National Wealth and National Income. It is derived from Census data, showing levels at census intervals from 1850 to the 1920's, and supplemented by estimates going back to 1790.

[2] Calculation based on data of 10 overlapping decades in S. Kuznets: *Uses of National Income, op. cit.*

POTENTIAL NET CAPITAL REQUIREMENTS

	Net Capital Requirements per Unit increase in Net Output. (*c*)	Rate of Growth of Net Output p.a. as % of Net Output (at comp. interest)	Annual Requirements of Net " Capital " Expenditure as % of Net Output. (*c* times rate of growth)
	(1)	(2)	(3)
U.K. (*a*) ..	3.3	2¼	7.4
U.K. (*b*) ..	3.3	1¾	5.8
U.S. ..	3.3	3	9.9
S.A. ..	2.5	4.5	11.25

Such a table of magnitudes—which reflects both empirical data and projections into the future from such data—can be used to illustrate and interpret many problems in international capital movements, including different interest levels in the different countries or in the same country at different stages of economic development. We will restrict ourselves here to touching upon the problems of savings required and of the level of interest. If each country were to supply its own capital requirements (i.e., were to prevent imports or exports of capital), the requirement rate for additional capital (Col. 3) represents also the required savings rate. *It is obvious that this rate is a function of the rate of progress to be achieved.* For instance, the U.S. would need a savings rate substantially higher than this country, and S.A. an even higher rate. Assume, on the other hand, the birthrate in the U.K. rising from 15 per thousand to 19 per thousand and this, together with a reduced death rate leading to a population increase of ¾% instead of ¼% p.a. As a consequence the capital requirements rate and, therefore, the required savings rate would rise from 7.4% to 9.1% of the annual net output (growth of income per head being assumed at 2% p.a.) The more economically progressive the society is the more rapidly the required savings rate will rise. Conversely, the rate of progress is in turn a function of the savings rate, though not of this alone. Keeping down the latter must create a tendency of stifling progress.

The rate of interest is the resultant of various risk rates and accommodation rates (some of which may be negative) and of a rate which may be called the basic interest rate, being the price which the marketing of the capital facilities, like the marketing of any other service, in a society of the " freely " competing type entails, even when no risk is involved. Like the price of any other service it is subject to fluctuations in accordance with the supply/demand position. It follows that in a society which remains steadily progressive, a decline of the savings rate due to whatever cause, will increase the difficulties—*ceteris paribus*—of balancing supply and demand of " capital " and will thereby create a tendency for the interest level to rise. Even a stable savings rate in a society progressing at a stable rate per head may have this effect, if the birth-rate rises, or even at stable birth-rates if the rate of increase of the adult population rises in consequence of high birth-rates in preceding periods, coupled with a falling death-rate.

It will be remembered that " capital " represents man-made producers' durable goods, buildings and construction, but not consumers' durable and semi-durable goods (other than buildings and construction). It follows that the *direction* of spending the National Income is of decisive importance for the maintenance of growth of the National Income and for the level of the interest rates. The assertions often made that the State is able to maintain income by spending the people's savings on whatever it likes and that ' the necessary offsetting of savings to maintain employment can take any form of spending if the spending is that of the State,' are obviously fallacious. We may go further and say that a notion is misleading which associates any rising interest level or any interest level higher than in previous periods or higher than in other countries with a decline of economic activity. A rising level of interest may well be the consequence of, and associated with, a rising rate of progress. It remains, of course, true that increases of interest rates which are due to inefficiency either in the marketing of capital or the limitation of risks are symptoms of waste and are a brake on progress just as is inefficiency in any other economic sector.

The findings in this article concern capital and income. It is not pretended that in every period income and employment are expanding apace or that every unit of income spent on goods ranked as " capital ", raises the level of income equally. The magnitudes found to prevail are long-term averages. In fact, the problem with which society is faced, whether it is " freely competing " or " centrally directed ", is that of *timing* the savings and *selecting* its composition as much as that of the *rate* of saving.

It follows that, in judging any person's merits in respect of contributing to the maintenance or expansion of the national welfare, the criterion is not power or ability of that person's (whether State or individual) to raise money, buth is ability (*a*) to *time* the saving adequately; (*b*) to *select* its composition; and (*c*) to maintain it at a *rate appropriate* to the desired rate of progress—and his ability to make the multitude perform these three tasks. If endowed with the ability or power to raise money but not with the ability to influence the flow of savings in a beneficial way, he may do good or harm, just as luck will have it, but he has no intrinsic merits in this respect in preference to anybody else.

A Note on the Post-War Industrialisation of "Backward" Countries and Centralist Planning

By YUAN-LI WU

THE NATURE OF EXTERNAL ECONOMIES

WHEN an entrepreneur makes a marginal increment of investment, he expects to get a certain product. This product may be no more than a part of the total benefit the increment of investment will actually bring forth; the rest may accrue to other people. Now if an industry (or firm) is expanding, its expansion may bring about a better organisation, understood in the widest sense, of all activities more or less remotely connected with the industry in question. This is the phenomenon of "external economies," an instance of the wider concept of a discrepancy between marginal social and marginal private products. If the benefit which the industry does not receive directly cannot affect it in any manner at all, we have what may be called external economies *simpliciter*. But the external economies may have further repercussions. Economies external to one industry are affected through re-organisation and qualitative changes of other industries,[1] which can take place only when those other industries respond to the initiative on the part of the expanded industry. In other words, these other industries will not remain stationary. New industries may spring up or develop out of old ones, while existing industries may expand. It will be profitable for at least some of the other industries to expand, because they are now getting more from their own investment than they used to. If they do expand, the same conditions that made the original external economies possible can perhaps be expected to produce economies external to the expanding industries. Some of these benefits will probably fall on the industry which made the first step towards expansion. If the various industries are so placed in relation to one another that the expansion of one of them benefits the others, it is not unlikely that the expansion of the others will in turn benefit the former. In so far as we can say that the later external economies are induced by the initial expansion of our industry, some of these benefits should be taken into account by that industry when expansion is considered. In other words, the private marginal product is greater than it may at first sight appear to be the case. Or, to put the statement differently, what may appear to be a direct benefit to other industries may yet be an indirect benefit to the industry in question. But it is important to bear in mind that this is so only when the other industries respond to the initial move. There must be, in other words, a synchronised process of expansion, a continuous extension of the market. Consequently, we are still dealing with a case in which the *external* factor plays a great part.

It seems that the "external" character of the advantages of expansion has made some people believe in the need of bringing them about by a "conscious" direction of the whole process. But "synchronised development" can easily become a cliché. Few would dispute the desirability of a synchronized economic development. Granting this, it is an easy step to demand the planning of synchronisation. Since all industries are inter-connected, to plan their synchronised development means to plan all industries, or, in short, to establish an economic order of centralist planning. Professor Allyn Young more than hinted at this argument[2] while Dr. Rosenstein-Rodan[3], in particular, has recently developed it.

YOUNG'S THEORY

To begin with, let us recapitulate briefly Professor Young's argument. Professor Young begins by pointing out the significance Adam Smith attaches to the relation between the division of labour and the extent of the market. The greater the extent of the market, the greater is the division of labour. The higher the degree of the division of labour, the larger the output. Division of labour reaches a higher degree when there is more specialisation and industrial differentiation which can be brought about by "large production," i.e., by the use of increasingly roundabout methods of production. Roundabout methods of production increase output, and may themselves be regarded as a special form of the division of labour. Industrial specialisation and differentation lead to better organisation of the market, and fall under the Marshallian category of external economies. But the extent of the market acts as a brake on the process of expansion. Hence it must be enlarged. It is continuously enlarged when expansion is synchronised. The remaining question to be answered is, therefore: Can this synchronisation be expected to occur spontaneously, or must it be planned?

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[1] Economies external to an industry when it expands are not equal to the sum total of all the internal economies obtained by other industries. There is a qualitative as well as a quantitative aspect to be considered.

[2] Cf. "Increasing Returns and Economic Progress," *Economic Journal*, 1928.

[3] P. N. Rosenstein-Rodan, "Problems of Industrialisation of Eastern and South-Eastern Europe," *Economic Journal*, 1943.

According to Professor Young, "Every important advance in the organisation of production, regardless of whether it is based upon anything which, in a narrow or technical sense, would be called a new 'invention,' or involves a further application of the fruits of scientific progress to industry, alters the conditions of industrial activity and initiates responses elsewhere in the industrial structure which in turn have a further unsettling effect. Thus change becomes progressive and propagates itself in a cumulative way."

The process is facilitated by the rôle played by ". . . reciprocal demand when the commodities exchanged are produced competitively under conditions of increasing returns and when the demand for each commodity is elastic, in the special sense that a small increase in supply will be attended by an increase in the amounts of other commodities which can be had in exchange for it. Under such conditions an increase in the supply of one commodity *is* an increase in the demand for other commodities, and it must be supposed that every increase in demand will evoke an increase in supply. The rate at which any one industry grows is conditioned by the rate at which other industries grow, but since the elasticity of demand and of supply will differ for different products, some industries will grow faster than others. Even with a stationary population and in the absence of new discoveries in pure or applied science there are no limits to the process of expansion except the limits beyond which demand is not elastic and returns do not increase."[1] So it seems that in Professor Young's opinion, the process can be entirely spontaneous. Given the favourable conditions, it will unfold itself in time. On the other hand, if the conditions are absent or insufficient, the process may not be able to continue for long, or will not start at all.[2]

The opportunity of the planner in this matter, it seems, is not to initiate such a process, but merely to speed it up. However, Young advises *against* such speeding-up of the process. In the first place, the human material used is resistant to change. Secondly, time is required for the accumulation of capital. "An industrial dictator, with foresight and knowledge, could hasten the pace somewhat, but he could not achieve an Aladdin-like transformation of a country's industry so as to reap the fruits of a half-century's progress in a few years."[3] The cost of forcing the "process," Young realises, is great, and involves imponderables in terms of human suffering.

DR. ROSENSTEIN-RODAN'S THREE LINES OF THOUGHT

Dr. Rosenstein-Rodan's arguments are not as straightforward as Young's. An analysis of them reveals three component factors. First, there is the basic fact that industries are complementary, and that when they are simultaneously developed, the progress of one tends to spur on that of another. Secondly, there is the realisation that under a rational economic order the State must play an important rôle—which need not be totalitarian planning—*e.g.*, in the training of personnel, the elimination of political risks, the adoption of measures to induce foreign investment, *etc.* Thirdly, there is evidently implied a somewhat uncritical faith in the economic efficiency of centralist planning. So far as the first point is concerned, Dr. Rosenstein-Rodan follows Young's footsteps, and voices an opinion the validity of which few would dispute. From the second line of thought, and in conjunction with the first, Dr. Rosenstein-Rodan might derive several clear directives which might indicate the structure of a rational economic order. But instead of this Dr. Rosenstein-Rodan allows himself to be misled by his faith in centralist planning which he tries to justify by arguments the validity of which is no longer beyond doubt.

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[1] Young, *op. cit.*, pp. 533–534.

[2] Demand for some commodities may be inelastic; there may be natural scarcities, etc. These factors may interrupt the process of expansion.

[3] Young, *ibid.*

The Complementary and Simultaneous Development of Industries

First, Dr. Rosenstein-Rodan thinks that it is necessary to have centralist planning in order to secure external economies in the wider sense, *i.e.*, in order to assure a complementary and simultaneous development of the various industries. This is actually in direct opposition to Professor Young's view. From what we have said above we can see that there is a confusion of the two types of external economies. Dr. Rosenstein-Rodan seems to think that private entrepreneurs invariably fail to see that the profits they derive from the expansion of their own enterprises are not confined to the direct benefits; namely, the immediate returns and the internal economies, if any. But there is no reason to believe that entrepreneurs will remain, at any rate for long, unaware of the subsequent advantages their enterprises will enjoy through simultaneous expansion. If they do recognise this fact, they will certainly also realise that a simultaneous expansion of all industries implies an expansion of their own. External economies tend to generate further external economies. The profit motive is surely sufficient to make entrepreneurs appreciate the advantages and to initiate and maintain the continuous and cumulative expansion of the complementary industries until no further economies can be obtained or other obstacles arise. Private calculation and self-interest should be sufficiently strong, even in " undeveloped " and poor countries, to bring this about.

Dr. Rosenstein-Rodan does not explicitly state that his sole criterion is the maximisation of economic welfare. But we can probably assume that he has this criterion in mind. If so, then he is obviously over-optimistic in his conception of the ease with which consumers' wants can be gauged. We should readily admit that there is a certain minimum of common needs for all people in the sense that to keep body and soul together there is a minimum requirement for every person of so many calories and so many cubic feet of fresh air per day. But it is not easy to translate these terms into definite amounts of commodities people will demand when they are free to choose. Not even in a poor and " undeveloped " community can this be easily done without the help of the market mechanism.

It seems fairly clear that Dr. Rosenstein-Rodan does not contemplate any kind of " competitive socialism." Can he then have in mind a *Zentralverwaltungswirtschaft*,[1] which would be incompatible with the welfare criterion? Probably he would not advocate such a system. But these two possibilities having been eliminated, what is the exact rôle of the State or the Investment Trust, and how are its powers to be defined in bringing about the complementary and simultaneous expansion of all industries? Are the capital structures of all industries to be centrally planned; and if so, does not this amount to smuggling in by the back door the system of totalitarian planning rejected through the front gate?

Furthermore, the expansion of industries is concomitant with industrial differentiation. The latter may lead to a higher degree of specialisation in management with its obvious advantages. It also lends itself probably to a better geographical distribution of industries. " A better combination of advantages of location, with a smaller element of compromise can be had by the more specialised industries."[2] The full realisation of these advantages, however, requires an increasing particularisation of knowledge. It is clear that the central authority can hardly be expected sufficiently to know the relevant particular facts in order to take advantage of the possibilities opened up by industrial differentiation and specialisation. Moreover, in the degree that differentiation and specialisation would be brought about by individual entrepreneurs during a process of simultaneous expansion only in anticipation of the full realisation of these advantages, in that degree the removal of the latter under centralist planning makes expansion less advantageous and prevents the effects of a greater division of labour from being entirely worked out. Dr. Rosenstein-Rodan, however, does not mention this point. We can only explain this by his assumption that the central authority, in all circumstances, necessarily possesses more relevant knowledge than the individual could possess.

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[1] Literally, this means a " central administrative economy." For want of a better and more compact term, we have decided to keep the German word, which is at least logical and unambiguous, in preference to " central planning," as the latter may convey different ideas to different people.

[2] Young, *op. cit.*, pp. 538–539.

THE POSSIBILITY OF A MODEL DEVELOPMENT SCHEME AND OTHER STIMULATING ACTIVITIES OF THE STATE

All this, of course, does not mean that the State need not do anything whatsoever in all circumstances in order to assure the simultaneous development of industries. It is conceivable that in an undeveloped economy ignorance may be so great among private entrepreneurs that they would really fail to see the advantages of a simultaneous and complementary expansion of industries. Granted that such a state of affairs may exist, it is, however, perfectly reasonable to assume that it could not last long once someone had shown the way. The State could, therefore, provide guidance by the dissemination of knowledge. If it is not content with this activity, it might initiate a model complementary economic development within the larger economy without subjecting the latter to centralist planning. If the economy is sufficiently large the model experiment could take advantage of the internal economies of large-scale production without running the risk of engulfing the whole economy in the attempt to establish itself. In other words, a multi-purpose development scheme comparable to the T.V.A. might be started to show the way.[1] Once this has been done, it seems highly probable that individual entrepreneurs will catch up in a very short time.

The State can further expedite the simultaneous development of different industries in another way. There are certain activities which can benefit almost all industries and which, for one reason or another have to be undertaken by the State. The announcement of a definite policy of keeping such activities in step with the degree of the economic development of all industries may have the effect of stimulating expansion in those fields where the entrepreneurs are somewhat lethargic. It might be even more effective if such activities were always kept slightly ahead of the stage of development and conducted in anticipation of further development. To illustrate our point, let us assume that road building is an activity of this nature. An official announcement, for instance, that so many miles of new roads will be built for every increase of one thousand H.P. of machinery in the district, or the construction of these new roads in anticipation of the increase, will probably induce many industries to expand, thus enabling them to secure other economies as well.

Hence there are at least two ways in which the State can stimulate lethargic entrepreneurs when the latter fail to initiate on their own a process of simultaneous and complementary expansion. In no case is centralist planning required or advisable: on the one hand, if adopted, it would not in fact secure all the advantages Dr. Rosenstein-Rodan and others believe it could secure; on the other hand, all the advantages that can be obtained at all through the intervention of the State can be obtained without centralist planning. The first important ground on which Dr. Rosenstein-Rodan bases his theory is, therefore, not established.

STATE CONTROL OF THE TRAINING OF PERSONNEL AND ITS DISADVANTAGES

Dr. Rosenstein-Rodan believes that centralist planning is also necessary because the State alone is willing and competent to train the personnel and skilled labour required for economic development. Whereas the individual entrepreneur runs a considerable risk if he invests in his employees in this manner, inasmuch as the better trained man may not continue to work for him after the training is completed, the State need not be affected by such considerations. In other words, the entrepreneur has to discount the increased productivity of the trained person by a risk premium. The marginal rate of his private returns on the investment in training men is in consequence smaller than the real marginal rate of returns represented by the increased productivity of the trained man. Although the discrepancy between the marginal private and marginal social products, in this case, is more potential than real, the threatening divergence is sufficient to deter the individual entrepreneur from making the optimum amount of investment. Furthermore, in an "undeveloped" community there is bound to be an acute shortage of trained personnel. It follows that competition for such men among entrepreneurs will be intense. All sorts of unfair practices may be resorted to in order to attract trained persons from their employers at any moment of time. The risk of losing one's employee after training is increased, and this at a time when the need for a large body of people with up-to-date knowledge is greatest. It is, therefore, possible that in an "undeveloped" community, investment in men may and probably will fall far short of the desired level.

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[1] See H. Finer, *The T.V.A. Lessons for International Application.*

Dr. Rosenstein-Rodan, therefore, rightly points to the need of some State control. But apart from training personnel for State-controlled industries this does not mean that the State should plan or direct all training facilities.

Presumably Dr. Rosenstein-Rodan would also put investment in research or the acquisition of new knowledge under State planning. While this activity is not the same as investment in the " skilling " of ordinary labour, the objections to their being directed by the State are essentially similar. First, it does not seem that any central authority could be in possession of the necessary particular circumstances of time and place without which the potential productivities of the trainees cannot be surmised with reasonable accuracy. Secondly, the State might be tempted to adopt as a criterion of selection something which has nothing to do with the object in hand. It might maintain that skill above a certain level should be an attribute of the Aryan only, or that no one should become a master shipwright without being a champion of as admirable a cause as women's suffrage. While there is no *a priori* certainty attached to this factor, the political set-up of a country might in certain circumstances be conducive to such abuse. Thirdly, while training personnel for industry, the State may waste resources in an attempt to impose some particular creed on the trainees. The last two objections might in certain circumstances apply to the private entrepreneur as well if he chooses to train his own personnel or establish facilities for industrial training. But two important differences must not be overlooked. First, the individual entrepreneur may have to face competition from others. Secondly, the State can lay down certain rules to be observed by the entrepreneurs. These would not apply to the State itself.

State Subsidy of Training

The proper rôle for the State to play would seem to be the granting of subsidies to all entrepreneurs who undertake to train the personnel they require themselves. Other conditions being equal, the subsidy could perhaps be best based on the degree in which the trainer is likely to lose the future service of the trainee, *i.e.*, the degree of probability in which the increased productivity of the trained personnel will benefit people other than the trainer. This particular risk can perhaps be measured by the frequency with which different groups of skilled labour change their jobs. The subsidy should then be a definite sum per trainee, the sum varying with the category the latter belongs to. Since the discrepancies between the marginal social and the marginal private products may vary a great deal as time goes on and also with different occupations, industries, and regional groups, if the optimum point is aimed at, a very complicated schedule of subsidies would have to be worked out. As in all such cases the State can but aim at the closest approximation to the ideal.

Guarantees of Profit and Loans and the Elimination of Political Risks

Let us now turn to Dr. Rosenstein-Rodan's third argument for centralist planning. He favours it because the State alone can increase the supply of capital by borrowing from abroad. In the development of a " backward " country, the people themselves cannot be expected to save enough to make high-speed development possible. Investment from abroad has to be induced. But conditions have changed so much since the nineteenth century that large-scale foreign investment to-day is no longer possible without the intervention of the State. " State supervision and guarantees can . . . substantially reduce risks."[1]

Dr. Rosenstein-Rodan's view on the importance of security in foreign investment and, conversely, on the adverse effect of insecurity on foreign investment in the past decades, is certainly shared by many people. While in the nineteenth century it was generally recognised that certain things were " not done," this is no longer so to-day. Presumably Dr. Rosenstein-Rodan means that investors to-day cannot be sure that foreign entrepreneurs will honour their pledges and will not try to evade payments somehow or other. But would it not be more correct to say that the risks of foreign investment have increased mainly *in consequence* of the intervention of the State? Would it not be more correct to say that governments, perhaps even to a greater extent than business, no longer accept the view that certain things are " not done " ?

Guarantees of profit or interest payments cannot be given to foreign investors unless the guarantor, *i.e.*, the government, takes an active part in the various industries in which foreign capital is invested. If the fact that guarantees are being given tends to make foreign investors less careful in the selection of fields for investment, then, unless special care is taken by the government in abstaining from the guarantee of risky enterprises, the possibility of the government's being called upon to honour its pledges will be increased. Assuming that no external pressure is brought to bear by the government on the free play of economic forces, which determine the profitability of an enterprise, the government is unable to estimate profitability without assuming the responsibility of acquiring all relevant particular knowledge which the entrepreneur would himself have to possess. Inability to honour guarantees would put the government under the pressure of foreign bond-holders and other investors. This means that the government would be exposed to political pressure from abroad; and political pressure from abroad involves political risks which government guarantees are supposed to eliminate.

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[1] Rosenstein-Rodan, *op. cit.*, p. 204.

At the same time, political pressure will also be exerted on the government by industries which hope to attract foreign investment. If government guarantees are at all effective in inducing foreign investment, there is bound to be strong competition among various groups for this favour. A tug-of-war among pressure groups for government guarantees of the returns on capital secured from abroad can hardly be conducive to the making of independent decisions based solely on objective estimates of the prospects of different enterprises. It would hardly seem an exaggeration to say that a situation like this, in which the government is subject to heavy pressure from inside as well as from outside, is full of political risks which would eventually destroy whatever general security government guarantees may have tended to create.

The Reduction of Risk through Better Information

However, the fact that owing to ignorance of actual conditions a higher risk premium may be allowed for by foreign investors than is necessary is an important one, and anything that can eliminate or diminish ignorance, and hence reduce the risk premium, should be useful. The proper solution might be the creation of an information service which would supply data on the economic conditions of the country and on the main factors affecting the profitability, etc., of the various industries. By organising and disseminating reliable information, investors' ignorance could be reduced. While an information service has other functions to fulfil, the immediate result would be twofold. On the one hand, more investments will be made in fields where through ignorance risk has previously been exaggerated. On the other hand, less will be invested in fields where through ignorance risk has previously been underestimated. Provided, of course, that there exists a stable government, foreign investors will find far more security in their investments than government guarantees of profits can afford them, and will realise that they only have to bear the risk of enterprise and nothing else. The increased supply of capital will lessen the burden of accumulation at home. Security and profitability will in time attract more foreign investment, and the process can continue with increasing magnitude as accurate knowledge becomes more widespread.

Imperialism, Foreign Investment and the Neutrality of the State

Apart from the immediate possibility that welfare considerations may be jettisoned by the government when it is subjected to political pressure from foreign investors as well as the country's own pressure groups, there is the danger of foreign imperialism.[1] Imperialism endangers the economic welfare of the peoples upon whom it is imposed. Nor is it a mere bogey when large investments have been made by an economically advanced country in a " backward " country. The danger can be reduced by making investments secure and profitable, and, at the same time, by making the position of at least the borrowers' government as unentangled and neutral as possible. The guarantee system, to say the least, does not necessarily satisfy the first condition, and definitely falls far short of the second. On the other hand, our suggestion reasonably satisfies the first condition, and does as much as is possible to fulfil the second condition.

To the extent that the risk of political pressure being brought to bear upon the debtor country would still remain even if our suggestion were followed, it should be realised that this risk is inherent in investments by one country in another. Both foreign investment *via* the State, and foreign investment in direct co-operation with individual entrepreneurs or in the form of direct foreign undertakings, cannot free the process entirely from this risk. Nor would the representation of the creditor and debtor countries on Dr. Rosenstein-Rodan's Eastern Europe Investment Trust solve the problem, so long as countries are represented as creditors and debtors. As we see it, the satisfactory answer lies in the internationalisation and commercialisation of foreign investment under a rational economic order in which economic power is tamed. In other words, what seems to be required is an international investment trust organised on a commercial basis, but under the control and supervision of a World Economic Council.

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[1] Cf., for instance, Hobson, *Imperialism : A Study*; and Lenin, *Imperialism*.

Book Reviews

The Pattern of Corporate Financial Structure. A Cross Section of Manufacturing, Mining, Trade and Construction, 1937. By WALTER A. CHUDSON. National Bureau of Economic Research. 1945. xiv + 148 pp. $2.00.

Corporate Cash Balances 1914–43. Manufacturing and Trade. By FRIEDRICH A. LUTZ. National Bureau of Economic Research. 1945. ix + 132 pp. $2.00.

These two small volumes are respectively the seventh and eighth in the series of Studies in Business Financing undertaken by the National Bureau of Economic Research with the help of grants from the Association of Reserve City Bankers and the Rockefeller Foundation. I have not had the opportunity of seeing the first six volumes of this series, but, if they are as rich in information as the two latest, the series should provide a magnificent source of raw materials, both for those concerned with running our economic system and for those in search of arguments to throw at the heads of rival theorists.

These two books sink two exploration trenches, at right angles to each other, the one across and the other along the course of United States business life during the past thirty years. In the first Dr. Chudson makes use of data, largely unpublished, provided by the Bureau of Internal Revenue and the Securities and Exchange Commission, to give an analysis of the financial statements for 1937 of about 80,000 non-financial corporations. Each of the dozen major groups into which balance sheet items are divided is expressed as a percentage (*a*) of total assets and (*b*) of total sales. These ratios are examined for about sixty different product groups, showing separate results for corporations working at a profit and for those working at a loss, and also for ten different sizes of firm in each of ten larger product groups, again distinguishing profitable from unprofitable businesses. The enormous number of individual results so obtained are presented mainly in two series of most ingeniously designed charts, though there are also a considerable number of special charts and tables. The calculating and tabulating work involved, undertaken largely by the staff of the National Bureau of Economic Research, must have been immense.

Few clear-cut generalisations emerge from the analysis by product groups. It would appear that, while the nature of the industry has a considerable influence on the distribution of the assets, its effect on the distribution of liabilities is largely outweighed by chance variations between one firm and another and, though sometimes perceptible, is considerably smaller than might have been expected.

More conclusions can be drawn from the analysis by size of firm and ratio to total assets. Large firms make larger profits than smaller ones, are more liquid despite a lower cash ratio, give and receive less trade credit, borrow much less from banks, and have smaller capitals but larger " net worths ". The split between profitable and unprofitable firms follows much the same lines. Profitable firms hold more cash and other liquid assets, extend much the same amount of trade credit as do the unprofitable firms but borrow less on short-term both from banks and trade creditors, and have smaller capitals but larger " net worths ".

The analysis by ratio to sales suffers from the general difficulty that the sales of a vertically integrated firm are much smaller in proportion to its total activity than the sales of a firm engaged upon only one stage of production. Therefore when balance sheet items are expressed as a ratio of sales, highly integrated concerns show a deceptively low rate of turnover. Thus large corporations, although generally believed to be more efficient than small in their management of stocks, apparently have a lower rate of inventory turnover. A better criterion of a firm's output, if available, would surely be the " value added " of the British Industrial Census—sales less purchases of raw materials and semi-manufactures.

Dr. Lutz, though not attempting to handle the masses of data used by Dr. Chudson, has set himself what is in some ways an even more difficult task in attempting to follow the movements of the liquid assets of American industry over a period of thirty years. He has carried out his investigation by the use of four main groups of samples—large manufacturing corporations, medium and small manufacturing corporations, large trading corporations and medium and small trading corporations. The first sample is a large one, representing something like 25 per cent. of all manufacturing corporations with total assets of $5 Mn. and over, but for the other groups much smaller samples have had to be used, those for medium and small firms being drawn from the files of the Wisconsin Tax Commission. However, Dr. Lutz considers that the results obtained are sufficiently representative to be of general application.

The investigation was undertaken for two main purposes. First, to examine the movements in corporate cash reserves during the course of the trade cycle; and second, to examine the changes in business liquidity during the present war, with particular reference to capacity for financing post-war reconversion.

The results of Dr. Lutz's investigation enable him to give a fairly clear answer to the first question. During the period 1926–36 the proportionate movements in the cash balances of all non-financial corporations paralleled very closely the movements in the country's total stock of means payment, so that " corporations neither gained cash at the expense of other cash-holders nor lost cash to the rest of the cash-holders in any marked degree ". Since, however, the value of sales fell in the depression considerably more than the total means of payment, the ratio of corporation cash reserves to sales increased, and, on the assumption that cash requirements fell proportionately with sales, a surplus of cash over requirements appeared. This surplus, which Dr. Lutz calls " free cash ", was mainly held by large corporations, partly because it was less depleted by trading losses than in the smaller corporations, and partly because the smaller corporations owed much more to the banks and used most of such " free cash " as they had in paying off their loans. In the 1921 depression, when all sizes of firms were heavily indebted to the banks, the phenomenon of " free cash " did not appear.

The answer to the second question is more ambiguous. Large manufacturing firms increased their cash reserves between 1939 and 1943 by only 47 per cent., as compared with a rise of 78 per cent. in the means of payment, and of over 120 per cent. in their sales, and even after allowing for a marked increase in their holding of marketable (mainly government) securities, they were appreciably less liquid than in 1939. Cash reserves of the smaller manufacturing corporations, on the other hand, rose by over 200 per cent. between 1940 and 1943, while trading corporations, though their cash reserves rose only in about the same proportion as the country's total means of payment, showed an increase of almost twentyfold in their holdings of securities. All types of non-financial corporations, therefore, showed a great increase in liquidity during the war with the exception of the large manufacturing corporations, and even these, as Dr. Lutz points out, may well have their liquidity increased at the end of the war by tax refunds and compensation for the cancellation of contracts. What effect the sales of large quantities of government securities to finance re-conversion might have on security markets lies outside Dr. Lutz's province; though he notes that " when the corporations actually did liquidate a large part of their holdings of marketable securities in 1919 prices began to break, particularly since commercial banks were not prepared to take over securities in large volume ".

F. W. PAISH.

Bureaucracy. By L. v. MISES. Yale University Press. 1944. viii + 125 pp. $2.

The author of this tract for the times has lost none of his cunning or courage. His case is not new, but his arguments are fresh, his language succinct, and his mode of presentation sometimes brilliant.

Bureaucracy is a function not of size but of State action. It is not to be detected by forms of organisation or measured by the length of its red tape, but must be seen as a response to the absence of the sanctions of profit and loss. The mammoth corporation is not bureaucratic unless it can evade the sanctions of the market. State organisation is bureaucratic whether it concerns the petty affairs of a parish or the grand affairs of a nation. When the economy becomes bureaucratic the power of money is replaced by the power of the politician. Open market competition is replaced by submerged political competition, *i.e.*, intrigue. The choice of the consumer gives way to the choice of the ruler.

All this is argued with high skill. But the book has two serious weaknesses. First, Mises dismisses the socialist " solutions " of the problem of calculation without analysis. Since his exposition of bureaucracy really depends on the impossibility of finding a socialist solution, a short statement on the lines of Professor Hayek's " Socialist Calculation: The Competitive Solution " (ECONOMICA, 1940) would have deepened his impression on readers new to the debate.

The second weakness is the worse. He dismisses Keynes with the totalitarians. Now it may be just to write off the Keynesians this way, for even the most sophisticated of them, *e.g.*, Mrs. Robinson, are capable of totalitarian excesses. And Keynes himself has often smiled on the barbarians—Keynes the journalist for example; Keynes the economic nationalist; and likewise the earlier Keynes who delivered himself of " The End of *Laissez-faire* ". In Britain that lecture was possibly a useful contribution to public debate, but in Berlin, where his audience yearned to have their superstitious hatred of liberal English economics confirmed, it was almost masochism. Yet there remains Keynes the master economist. It will not do for Mises to declare *tout court* that in a free market there is only frictional unemployment. The greatest need of modern popular liberalism is to restate itself in a form which makes room for the Keynes approach. This does not mean the Beveridge plan of covering totalitarianism with homage to freedom. It does mean amplifying older forms of analysis. Can we ask Mises, or perhaps Hayek or Roepke, to do this in popular form ?

A. A. SHENFIELD.

Cost Behavior and Price Policy: A Study Prepared by The Committee on Price Determination for the Conference on Price Research. National Bureau of Economic Research, New York. 1943. xix + 356 pp. $3.00.

The traditional theory of value based on the marginal analysis and the assumption of profit maximisation has received rude shocks in recent years. The pioneer investigation, conducted by Messrs. Hall and Hitch of Oxford, into the practical price policy of business men by an empirical approach has led to the disturbing conclusions that the majority of the entrepreneurs in the real world think in altogether different terms from the economists and that prices are frequently fixed by a rule-of-thumb method on the basis of unit prime cost rather than by a rational balancing of marginal revenue and marginal cost.[1] Similar opinion is expressed in such high quarter as Lord Keynes, who remarks in one of his recent articles that " indeed, it is rare for anyone but an economist to suppose that price is predominately governed by marginal cost. Most business men are surprised by the suggestion that it is a close calculation of short-period marginal cost or of marginal revenue which should dominate their price policies ".[2] Thus it appears that in spite of the developments of the elaborate analytical tools in the past century, we have to admit that economics has not yet satisfactorily solved its fundamental problem of how prices are determined. It seems that the whole theory of value and the theory of distribution which is built upon it call for an urgent overhaul, and that above all more empirical researches into the actual business practices of cost calculation and price fixing of the entrepreneurs in the real world are needed.

In the United States, where empirical research in economics is perhaps more vigorously pursued than anywhere else, the need of bridging the gap between theory and practice in price theory has now received its due recognition. A Committee on Price Determination was set up in 1938 under the Conference on Price Research sponsored by the National Bureau of Economic Research to investigate the possibilities of empirical research in the field of cost-price relationships and price determination. This book is the first report of the above Committee, which comprises a group of eminent American economists. Students interested in this field have reasons to be grateful to the authors for the clarification of the relevant concepts, the suggestions of methods and further possibilities of research, and particularly the comprehensive review of the relevant materials and existing studies, which, being scattered among the various journals and tracts, might otherwise be not easily accessible or remain unnoticed.

This book is divided into three parts. The first is of an introductory nature, in which the theoretical background relating to the nature of costs and prices is ably discussed. The second part, which is by far the most substantial, is devoted to the discussion of cost behaviour and includes chapters on such interesting topics as the allocation of fixed costs over time, cost-output functions, the allocation of costs among different products, selling costs, and the relationship between costs and the size of plants and firms. It is in this part that many interesting empirical studies come under review. At the end of each chapter, possibilities of further research are suggested. Broadly speaking, the suggestions for further research fall into two types. The first consists of direct empirical inquiries into the practices and techniques actually employed by the firms under different market conditions in their calculation of costs and in making decisions concerning the pricing of products, investments, etc. The second type consists of statistical derivation of the functional relationships between various variables that are supposed to underlie entrepreneurs' decisions, such as the cost-output function, selling cost-sales function and the relationship between the scale of plant or firm and costs. While one must admit that a knowledge of the general shapes of some of these functional relationships, e.g., the short period cost-output function, are of great significance to the price theory, it is sometimes difficult to share our author's optimism with respect to the feasibility of the econometric measurement of the functional relationship between two variables in isolation. The case I have particularly in mind is the selling cost-sales function. Apart from the extreme difficulty of the exact definition and demarcation of selling costs, which our authors well recognise, the selling cost-sales function relevant for the entrepreneur's decision is essentially subjective. Under oligopolistic market conditions, it also depends to a large extent upon the expectation of the entrepreneur in question as to what the reactions of his rivals to his own selling policy are likely to be, and such expectation may or may not turn out to be correct. Furthermore, since the effect of a major selling campaign is generally more or less enduring and cumulative, theoretically the selling cost-sales function will therefore shift bodily with every big selling effort. The significance of a statistically derived selling cost-sales function is rendered even more doubtful by the surveys of the Association of National Advertisers in the United States reported in this book, which show that the majority of the firms studied determine their advertising budgets not on the basis of any definite knowledge of, or assumption about, the selling cost-sales function, but on the basis of a convention as to what percentage of the sales proceeds should be spent on advertising (pp. 203–4). It seems to me therefore that in this case at least, research workers will be more amply repaid for their effort, if they direct their investigation to the way in which decisions concerning selling costs are made by actual business men and to the relation between selling costs and price policy rather than attempt to approximate the selling cost-sales function by elaborate econometric techniques from the ex-post data.

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[1] " Price Theory and Business Policy Behaviour ", *Oxford Economic Paper*, No. 2.

[2] " Relative Movements of Real Wages and Output ", *Economic Journal*, Nov., 1939.

In part three, which consists of a single chapter, the vital problem of price determination and the relation between cost and price is dealt with. The suggestion of Messrs. Hall and Hitch that the typical individual entrepreneur in the real world generally assumes an imagined demand curve with a kink at the prevailing price is endorsed by the authors in the remark: " There is no doubt that this is a realistic picture of the demand situation as envisaged by individual firms in a great number of industrial markets a large part of the time. It goes a long way toward explaining some important aspects of industrial price behavior " (p. 278). But the reservation is soon added that " as a working hypothesis, however, it is probably limited to industrial markets which have attained something like long-run stability in the sense that the demand is mainly for replacement and the entry of new firms is unimportant ". The " full cost principle " is also recognised as the basis of pricing " in fairly wide use among business firms in this country as well as in England " (p. 286). However, the ambiguity of the term full cost and the probable limitation of the application of this principle to particular kinds of markets are not overlooked. Clearly only more comprehensive empirical inquiries of the actual pricing practices and techniques can give us more light.

One cannot help finishing reading this book with a strengthened conviction that more and more empirical studies are needed to provide us with a firm foundation of facts on which to reconstruct a new workable theory of prices. Now that the National Bureau of Economic Research has turned its attention to this urgent need, we hope that it will soon follow up this general report with a series of concrete studies. It may well be the opening of a new chapter in economic research.

S. C. TSIANG.

Bibliography of the Published Writings of John Stuart Mill. Edited from his Manuscript with Corrections and Notes by Ney Macminn, J. R. Hainds, and James McNab McCrimmon. (Northwestern University Studies in the Humanities No. 12.) Northwestern University, Evanston, Ill. 1945. xiv, + 101 pp.

This carefully prepared edition of John Stuart Mill's own hand-list of his published writings will prove helpful and informative to many users, even though from a practical point of view they may often regret that the editors have all too scrupulously adhered to the canons of literary editing of manuscripts works instead of using the document as a basis for a final bibliography of Mill's work. With the single exception of one item entered twice in succession in the original, the list is completely reprinted from the notebook in the possession of the Library of the London School of Economics with all its errors, gaps, and deviations from the chronological order, although only the first of the thirty-nine pages of the MS. is in Mill's own hand. The rest is in a hand which might be Helen Taylor's if it were not for the frequent misspellings of proper names (p. 58: " De Luciney " for " De Quincey "; p. 71: "Bunghan " for " Brougham ") which must have been household words to Helen Taylor. The nature of these mistakes and the occasional use of the first person singular seem to make it certain that the greater part of the notebook must have been transcribed by a rather inferior amanuensis from notes provided by Mill.

To the reproduction of the original entry the editors have added in each case a correct bibliographical description of the item and a brief note on its contents. They have not, however, added any of the missing items (and after 1865 Mill's letters and speeches which appeared in print are systematically omitted), or assisted the user in establishing the true chronological order in the very numerous instances where items are entered out of place, or provided the much needed index to titles, proper names, subjects and periodicals. Except for a few incomplete references in the Preface, posthumous publications[1] or editions have not been included and even some major omissions in the original MS. are mentioned only in a footnote to the Preface.

The editor's bibliographical descriptions seem reasonably complete and accurate and some of the few gaps caused by the circumstances of the war can now be filled in: The article on " Paper Currency and Commercial Distress ", listed on p. 7, appeared (according to notes taken some years ago from the copy in the Library of the London Guildhall, now presumably destroyed) in the second volume of the *Parliamentary History and Review: Containing Reports of the Proceedings of the two Houses of Parliament during the Sessions of 1826–7, Geo. IV*, London, 1826, pp. 630–662; the article on "Corporation and Church Property " from *The Jurist* of 1833, listed on p. 25, was reprinted as a separate pamphlet by S. Sweet, Stevens & Son, and A. Maxwell, and a copy is among the part of Mill's Library preserved in Somerville College, Oxford; the full title of the brochure mentioned on p. 79 is *Remarks on Mr. Fitzroy's Bill for the more effectual Prevention of Assaults on Women and Children*, an unsigned pamphlet of 12 pages marked " For private distribution "; the *Paper on the Reorganisation of the Civil Service*, listed on p. 68, exists also as a separate reprint of 9 pages, dated May 22, 1854, issued London, 1855; and the unidentified pamphlet mentioned in the footnote on p. 96, in which Mill's review of Henry Romilly's *Public Responsibility and the Vote by Ballot* was reprinted from *The Reader*, is the second edition of the same pamphlet by Romilly " to which are appended a letter from John Stuart Mill, M.P., to the editor of the *Reader*, 29th April, 1865, and observations thereupon. London 1867 ", a copy of which is also in Somerville College, Oxford. There are one or two minor slips in the bibliographical descriptions, such as on p. 43, where the misspelling of " Senin " for " Senior " is twice repeated from the original entry, and on p. 94 where the misspelling " Cairne " for " Cairnes " is similarly followed.

The difficult task of giving in a few lines an idea of the subject of the paper in question is, on the whole, successfully performed, although the relative importance of the different items is not always appreciated. Mill's celebrated review of W. T. Thornton's *On Labour*, e.g., is discussed without any mention of the fact that it contains the sensational retraction of the wage-fund theory.

[1] To the list of posthumous publications given in a footnote on p. xii at least the following ought to be added: The essay *On Social Freedom*, recently again reprinted by Columbia University Press, 1941, the comments on Robert Browning's *Pauline*, published in *The Life of Robert Browning* by H. W. Griffin and H. C. Minchin, 1910, and one more early speech, presumably from Professor Laski's collection, which appeared under the title " On the present state of literature " in *The Adelphi*, Vol. I, No. 8, January, 1924.

Altogether the *Bibliography* lists over 700 items, the larger part of them, of course, short and anonymous contributions to the daily or periodical press. The contribution of the publication consists mainly in making this wealth of material on the intellectual development of Mill for the first time accessible, and every future student of Mill will be indebted to the editors for the pains they have taken in making available the only source from which many of those minor writings of Mill could ever have been identified.

F. A. HAYEK.

The Structure of Soviet Wages. A Study in Socialist Economics. By ABRAM BERGSON. (Harvard Economic Studies, Vol. LXXVI.) Cambridge, Mass., Harvard University Press. London, Oxford University Press. 1944. xii + 255 pp. 20s.

Among recent detailed studies of particular aspects of the Russian economy, Mr. Bergson's investigation of the wage structure is one of the most important and instructive. If his careful and conscientious survey has not brought as much information to light as one might wish, this is not his fault. He has spared no effort and has thoroughly searched the Russian literature and analysed and presented the results in a manner both clear and precise.

The study is largely concerned with wage differentials in particular industries and in industry as a whole. The statistical technique employed consists mainly in computing quartile ratios as the measures of frequency distribution of wage earners classified according to earnings, both for all wage earners and industry by industry. Though the ratio of the first and the third quartiles of the frequency distribution picks out only one aspect, the author is probably justified in claiming it as " an intuitively acceptable measure of inequality ".

The careful discussion of the significance of money rewards in the Soviet economy shows the great and increasing extent to which policy has been driven to rely on the money incentive and how even most of the much advertised non-pecuniary incentives, such as the various orders and " Hero " titles, are regularly accompanied by substantial financial benefits. In general, while during the first decade of Soviet rule a deliberate and in some measure successful effort was made to reduce inequalities in earnings, this tendency has since been largely reversed, though in 1934 (the last year for which detailed information is available) the distribution was still more equalitarian than in 1914.

Probably the most interesting part of the book is a comparison of wage distribution in Russia in 1928 (the year among those for which information is available when the maximum of equalitarianism had been achieved) and the corresponding situation in the United States in 1904. The author's conclusion is " that in general the inequality of earnings among Soviet industrial workers in 1928 was closely proximate to that among American industrial workers in 1904 " (pp. 90–92, cf. also p. 208) and that it must be concluded " that Soviet wages varied in accord with capitalist principles " (p. 96).

There is much incidental information in the book on such subjects as rationing, housing, education, and income tax. There are also some, though limited, data on the relations between the incomes of wage earners and salaried employees. Other important sidelights on special aspects of the Soviet economy are the emphasis on the fact that " it is *money* costs that the administrators seeks to economise " in the use of manpower (p. 138), and the conclusion that " in all Soviet industry in 1928, of the total horsepower capacity of prime movers, the age of installation of which was known, over four-fifths had been installed prior to the war " (of 1914).

It is a long time since so much solid information on any aspect of the Soviet economy has last become available between the covers of one book.

F. A. HAYEK.

Indian Economy during the War. By L. C. JAIN. Lahore. 1944. Second edition. 137 pp. Rs. 3/4.

An account of the effects of the war on Indian Economy by Professor Jain naturally arouses great expectations. It was courageous to tackle the subject before the war was even over, and before all the information necessary for a full account of wartime developments, let alone for an appraisal of the policies pursued, had become available. That the book has met a felt need is shown by the fact that a second edition was called for within a few months of the publication of the first, but it must be said at once that in certain respects it is disappointing. Allowance must obviously be made for the necessarily incomplete and tentative character of a book written under the conditions mentioned above, but the chief fault of Professor Jain's work is that he himself seems to overlook these conditions. He is insufficiently tentative: in particular his criticisms of Governmental wartime measures—whether or not they eventually prove to be correct—are based upon insufficient analysis of the problems involved, whilst his conclusions about economic trends and the effects of the policies pursued are based upon very scanty evidence indeed.

The work originated in six lectures delivered at Patna University in 1942 and 1943, which were recast and brought up to date for publication in March, 1944, the second (revised) edition appearing in June, 1944. The book bears strong marks of its origin, and is not sufficiently systematic, balanced or thorough to be considered a permanent contribution to the subject. What it does is to give a concise and lucid commentary on five aspects of India's war-economy; i.e., Agriculture, Industries, Trade, the Money Market, and Finance. The final chapter summarises the author's main conclusions and strongly urges the necessity to plan now for post-war economic reconstruction. What it fails to do is to provide a well-balanced picture of India at war—of the main trends of development, of the principal war measures and of the effects of war on the various aspects of Indian economy. Many vital subjects are either entirely omitted or are discussed merely incidentally, in a very superficial manner. For instance, an entirely inadequate account is given of India's contribution to the war effort on the economic—and specifically on the industrial—side. It is true that no complete account can yet be given, but many facts and even figures are known (and have been quoted in various official publications) and it does less than justice to India to omit all reference to these facts. In general, industrial developments since 1939 receive hardly any attention. Little more than a page is given to the Iron and Steel Industry, a mere passing reference being made to " four companies " producing steel. The English reader, at least, would like to know what companies these are, where they are situated, what they produce and so on. No adequate analysis is given of the causes and course of the Bengal Famine of 1943, let alone of the measures already taken to prevent a recurrence. Has rationing been introduced? Where and when? With what results? Railway developments are barely mentioned—apart from the fact that railway capital has been repatriated. Does this mean that nationalisation has occurred? No light is thrown on this subject. Above all, as already indicated, he fails to provide the full analysis of the facts and problems necessary to substantiate his almost unrelieved condemnation of Governmental actions. His criticisms may be warranted. Personally I tend to agree with his criticisms of the failure to check inflation, or to prevent the disastrous famine of 1943, but he certainly has not reproduced here the facts and arguments necessary to support his conclusions.

Let us, for instance, take the subject of post-war economic reconstruction. In this connection he refers to the pre-war Congress Planning Enquiry, and to the Bombay Plan of 1944. But he says no single word about the reconstruction enquiries and projects of the Government of India, in spite of the very considerable and commendable achievements recorded in official reports and summarised in the annual reports of the Reconstruction Committee of Council (the second of these was published in India in 1944), and in spite of the fact that the publication of the " Bombay Plan " was followed (in April, 1944) by the appointment of one of the authors of the Plan, Sir Ardeshir Dalal, as Minister for Economic Reconstruction. Again, no mention is made of what may prove to be one of India's greatest post-war assets—i.e., the technical training given to a considerable number of Indians, both in England and in India. Many other examples might be quoted of a similar nature. It would, however, be unfair to harp on omissions, when the subject tackled is so immense. But in recommending Professor Jain's book as a highly readable and stimulating introduction to a fascinating and most important subject, it is essential to warn the reader not to accept uncritically Professor Jain's unfinished picture.

VERA ANSTEY.

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The Economic Organisation of a P.O.W. Camp

By R. A. RADFORD

INTRODUCTION

AFTER allowance has been made for abnormal circumstances, the social institutions, ideas and habits of groups in the outside world are to be found reflected in a Prisoner of War Camp. It is an unusual but a vital society. Camp organisation and politics are matters of real concern to the inmates, as affecting their present and perhaps their future existences. Nor does this indicate any loss of proportion. No one pretends that camp matters are of any but local importance or of more than transient interest, but their importance there is great. They bulk large in a world of narrow horizons and it is suggested that any distortion of values lies rather in the minimisation than in the exaggeration of their importance. Human affairs are essentially practical matters and the measure of immediate effect on the lives of those directly concerned in them is to a large extent the criterion of their importance at that time and place. A prisoner can hold strong views on such subjects as whether or not all tinned meats shall be issued to individuals cold or be centrally cooked, without losing sight of the significance of the Atlantic Charter.

One aspect of social organisation is to be found in economic activity, and this, along with other manifestations of a group existence, is to be found in any P.O.W. camp. True, a prisoner is not dependent on his exertions for the provision of the necessaries, or even the luxuries of life, but through his economic activity, the exchange of goods and services, his standard of material comfort is considerably enhanced. And this is a serious matter to the prisoner: he is not "playing at shops" even though the small scale of the transactions and the simple expression of comfort and wants in terms of cigarettes and jam, razor blades and writing paper, make the urgency of those needs difficult to appreciate, even by an ex-prisoner of some three months' standing.

Nevertheless, it cannot be too strongly emphasised that economic activities do not bulk so large in prison society as they do in the larger world. There can be little production; as has been said the prisoner is independent of his exertions for the provision of the necessities and luxuries of life; the emphasis lies in exchange and the media of exchange. A prison camp is not to be compared with the seething crowd of higglers in a street market, any more than it is to be compared with the economic inertia of a family dinner table.

Naturally then, entertainment, academic and literary interests, games and discussions of the "other world" bulk larger in everyday life than they do in the life of more normal societies. But it would be wrong to underestimate the importance of economic activity. Everyone receives a roughly equal share of essentials; it is by trade that individual preferences are given expression and comfort increased. All at some time, and most people regularly, make exchanges of one sort or another.

Although a P.O.W. camp provides a living example of a simple economy which might be used as an alternative to the Robinson Crusoe economy beloved by the text-books, and its simplicity renders the demonstration of certain economic hypotheses both amusing and instructive, it is suggested that the principal significance is sociological. True, there is interest in observing the growth of economic institutions and customs in a brand new society, small and simple enough to prevent detail from obscuring the basic pattern and disequilibrium from obscuring the working of the system. But the essential interest lies in the universality and the spontaneity of this economic life; it came into existence not by conscious imitation but as a response to the immediate needs and circumstances. Any similarity between prison organisation and outside organisation arises from similar stimuli evoking similar responses.

The following is as brief an account of the essential data as may render the narrative intelligible. The camps of which the writer had experience were Oflags and consequently the economy was not complicated by payments for work by the detaining power. They consisted normally of between 1,200 and 2,500 people, housed in a number of separate but intercommunicating bungalows, one company of 200 or so to a building. Each company formed a group within the main organisation and inside the company the room and the messing syndicate, a voluntary and spontaneous group who fed together, formed the constituent units.

Between individuals there was active trading in all consumer goods and in some services. Most trading was for food against cigarettes or other foodstuffs, but cigarettes rose from the status of a normal commodity to that of currency. RMk.s existed but had no circulation save for gambling debts, as few articles could be purchased with them from the canteen.

Our supplies consisted of rations provided by the detaining power and (principally) the contents of Red Cross food parcels—tinned milk, jam, butter, biscuits, bully, chocolate, sugar, etc., and cigarettes. So far the supplies to each person were equal and regular. Private parcels of clothing, toilet requisites and cigarettes were also received, and here equality ceased owing to the different numbers despatched and the vagaries of the post. All these articles were the subject of trade and exchange.

The Development and Organisation of the Market

Very soon after capture people realised that it was both undesirable and unnecessary, in view of the limited size and the equality of supplies, to give away or to accept gifts of cigarettes or food. "Goodwill" developed into trading as a more equitable means of maximising individual satisfaction.

We reached a transit camp in Italy about a fortnight after capture and received ¼ of a Red Cross food parcel each a week later. At once exchanges, already established, multiplied in volume. Starting with simple direct barter, such as a non-smoker giving a smoker friend his cigarette issue in exchange for a chocolate ration, more complex exchanges soon became an accepted custom. Stories circulated of a padre who started off round the camp with a tin of cheese and five cigarettes and returned to his bed with a complete parcel in addition to his original cheese and cigarettes; the market was not yet perfect. Within a week or two, as the volume of trade grew, rough scales of exchange values came into existence. Sikhs, who had at first exchanged tinned beef for practically any other foodstuff, began to insist on jam and margarine. It was realised that a tin of jam was worth ½ lb. of margarine plus something else; that a cigarette issue was worth several chocolate issues, and a tin of diced carrots was worth practically nothing.

In this camp we did not visit other bungalows very much and prices varied from place to place; hence the germ of truth in the story of the itinerant priest. By the end of a month, when we reached our permanent camp, there was a lively trade in all commodities and their relative values were well known, and expressed not in terms of one another—one didn't quote bully in terms of sugar—but in terms of cigarettes. The cigarette became the standard of value. In the permanent camp people started by wandering through the bungalows calling their offers—"cheese for seven" (cigarettes)—and the hours after parcel issue were Bedlam. The inconveniences of this system soon led to its replacement by an Exchange and Mart notice board in every bungalow, where under the headings "name", "room number", "wanted" and "offered" sales and wants were advertised. When a deal went through, it was crossed off the board. The public and semi-permanent records of transactions led to cigarette prices being well known and thus tending to equality throughout the camp, although there were always opportunities for an astute trader to make a profit from arbitrage. With this development everyone, including non-smokers, was willing to sell for cigarettes, using them to buy at another time and place. Cigarettes became the normal currency, though, of course, barter was never extinguished.

The unity of the market and the prevalence of a single price varied directly with the general level of organisation and comfort in the camp. A transit camp was always chaotic and uncomfortable: people were overcrowded, no one knew where anyone else was living, and few took the trouble to find out. Organisation was too slender to include an Exchange and Mart board, and private advertisements were the most that appeared. Consequently a transit camp was not one market but many. The price of a tin of salmon is known to have varied by two cigarettes in 20 between one end of a hut and the other. Despite a high level of organisation in Italy, the market was morcellated in this manner at the first transit camp we reached after our removal to Germany in the autumn of 1943. In this camp—Stalag VIIA at Moosburg in Bavaria—there were up to 50,000 prisoners of all nationalities. French, Russians, Italians and Jugo-Slavs were free to move about within the camp: British and Americans were confined to their compounds, although a few cigarettes given to a sentry would always procure permission for one or two men to visit other compounds. The people who first visited the highly organised French trading centre, with its stalls and known prices, found coffee extract—relatively cheap among the tea-drinking English—commanding a fancy price in biscuits or cigarettes, and some enterprising people made small fortunes that way. (Incidentally we found out later that much of the coffee went " over the wire " and sold for phenomenal prices at black market cafés in Munich: some of the French prisoners were said to have made substantial sums in RMk.s. This was one of the few occasions on which our normally closed economy came into contact with other economic worlds.)

Eventually public opinion grew hostile to these monopoly profits—not everyone could make contact with the French—and trading with them was put on a regulated basis. Each group of beds was given a quota of articles to offer and the transaction was carried out by accredited representatives from the British compound, with monopoly rights. The same method was used for trading with sentries elsewhere, as in this trade secrecy and reasonable prices had a peculiar importance, but as is ever the case with regulated companies, the interloper proved too strong.

The permanent camps in Germany saw the highest level of commercial organisation. In addition to the Exchange and Mart notice boards, a shop was organised as a public utility, controlled by representatives of the Senior British Officer, on a no profit basis. People left their surplus clothing, toilet requisites and food there until they were sold at a fixed price in cigarettes. Only sales in cigarettes were accepted—there was no barter— and there was no higgling. For food at least there were standard prices: clothing is less homogeneous and the price was decided around a norm by the seller and the shop manager in agreement; shirts would average say 80, ranging from 60 to 120 according to quality and age. Of food, the shop carried small stocks for convenience; the capital was provided by a loan from the bulk store of Red Cross cigarettes and repaid by a small commission taken on the first transactions. Thus the cigarette attained its fullest currency status, and the market was almost completely unified.

It is thus to be seen that a market came into existence without labour or production. The B.R.C.S. may be considered as " Nature " of the text-book, and the articles of trade—food, clothing and cigarettes—as free gifts—land or manna. Despite this, and despite a roughly equal distribution of resources, a market came into spontaneous operation, and prices were fixed by the operation of supply and demand. It is difficult to reconcile this fact with the labour theory of value.

Actually there was an embryo labour market. Even when cigarettes were not scarce, there was usually some unlucky person willing to perform services for them. Laundrymen advertised at two cigarettes a garment. Battle-dress was scrubbed and pressed and a pair of trousers lent for the interim period for twelve. A good pastel portrait cost thirty or a tin of " Kam ". Odd tailoring and other jobs similarly had their prices.

There were also entrepreneurial services. There was a coffee stall owner who sold tea, coffee or cocoa at two cigarettes a cup, buying his raw materials at market prices and hiring labour to gather fuel and to stoke; he actually enjoyed the services of a chartered accountant at one stage. After a period of great prosperity he overreached himself and failed disastrously for several hundred cigarettes. Such large-scale private enterprise was rare but several middlemen or professional traders existed. The padre in Italy, or the men at Moosburg who opened trading relations with the French, are examples: the more subdivided the market, the less perfect the advertisement of prices, and the less stable the prices, the greater was the scope for these operators. One man capitalised his knowledge of Urdu by buying meat from the Sikhs and selling butter and jam in return: as his operations became better known more and more people entered this trade, prices in the Indian Wing approximated more nearly to those elsewhere, though to the end a " contact " among the Indians was valuable, as linguistic difficulties prevented the trade from being quite free. Some were specialists in the Indian trade, the food, clothing or even the watch trade. Middlemen traded on their own account or on commission. Price rings and agreements were suspected and the traders certainly co-operated. Nor did they welcome newcomers. Unfortunately the writer knows little of the workings of these people: public opinion was hostile and the professionals were usually of a retiring disposition.

One trader in food and cigarettes, operating in a period of dearth, enjoyed a high reputation. His capital, carefully saved, was originally about 50 cigarettes, with which he bought rations on issue days and held them until the price rose just before the next issue. He also picked up a little by arbitrage; several times a day he visited every Exchange or Mart notice board and took advantage of every discrepancy between prices of goods offered and wanted. His knowledge of prices, markets and names of those who had received cigarette parcels was phenomenal. By these means he kept himself smoking steadily—his profits—while his capital remained intact.

Sugar was issued on Saturday. about Tuesday two of us used to visit Sam and make a deal; as old customers he would advance as much of the price as he could spare then, and entered the transaction in a book. On Saturday morning he left cocoa tins on our beds for the ration, and picked them up on Saturday afternoon. We were hoping for a calendar at Christmas, but Sam failed too. He was left holding a big black treacle issue when the price fell, and in this weakened state was unable to withstand an unexpected arrival of parcels and the consequent price fluctuations. He paid in full, but from his capital. The next Tuesday, when I paid my usual visit he was out of business.

Credit entered into many, perhaps into most, transactions, in one form or another. Sam paid in advance as a rule for his purchases of future deliveries of sugar, but many buyers asked for credit, whether the commodity was sold spot or future. Naturally prices varied according to the terms of sale. A treacle ration might be advertised for four cigarettes now or five next week. And in the future market " bread now " was a vastly different thing from " bread Thursday ". Bread was issued on Thursday and Monday, four and three days' rations respectively, and by Wednesday and Sunday night it had risen at least one cigarette per ration, from seven to eight, by supper time. One man always saved a ration to sell then at the peak price: his offer of " bread now " stood out on the board among a number of " bread Monday's " fetching one or two less, or not selling at all—and he always smoked on Sunday night.

THE CIGARETTE CURRENCY

Although cigarettes as currency exhibited certain peculiarities, they performed all the functions of a metallic currency as a unit of account, as a measure of value and as a store of value, and shared most of its characteristics. They were homogeneous, reasonably durable, and of convenient size for the smallest or, in packets, for the largest transactions. Incidentally, they could be clipped or sweated by rolling them between the fingers so that tobacco fell out.

Cigarettes were also subject to the working of Gresham's Law. Certain brands were more popular than others as smokes, but for currency purposes a cigarette was a cigarette. Consequently buyers used the poorer qualities and the Shop rarely saw the more popular brands: cigarettes such as Churchman's No. 1 were rarely used for trading. At one time cigarettes hand-rolled from pipe tobacco began to circulate. Pipe tobacco was issued in lieu of cigarettes by the Red Cross at a rate of 25 cigarettes to the ounce and this rate was standard in exchanges, but an ounce would produce 30 home-made cigarettes. Naturally, people with machine-made cigarettes broke them down and re-rolled the tobacco, and the real cigarette virtually disappeared from the market. Hand-rolled cigarettes were not homogeneous and prices could no longer be quoted in them with safety: each cigarette was examined before it was accepted and thin ones were rejected, or extra demanded as a make-weight. For a time we suffered all the inconveniences of a debased currency.

Machine-made cigarettes were always universally acceptable, both for what they would buy and for themselves. It was this intrinsic value which gave rise to their principal disadvantage as currency, a disadvantage which exists, but to a far smaller extent, in the case of metallic currency;—that is, a strong demand for non-monetary purposes. Consequently our economy was repeatedly subject to deflation and to periods of monetary stringency. While the Red Cross issue of 50 or 25 cigarettes per man per week came in regularly, and while there were fair stocks held, the cigarette currency suited its purpose admirably. But when the issue was interrupted, stocks soon ran out, prices fell, trading declined in volume and became increasingly a matter of barter. This deflationary tendency was periodically offset by the sudden injection of new currency. Private cigarette parcels arrived in a trickle throughout the year, but the big numbers came in quarterly when the Red Cross received its allocation of transport. Several hundred thousand cigarettes might arrive in the space of a fortnight. Prices soared, and then began to fall, slowly at first but with increasing rapidity as stocks ran out, until the next big delivery. Most of our economic troubles could be attributed to this fundamental instability.

Price Movements

Many factors affected prices, the strongest and most noticeable being the periodical currency inflation and deflation described in the last paragraphs. The periodicity of this price cycle depended on cigarette and, to a far lesser extent, on food deliveries. At one time in the early days, before any private parcels had arrived and when there were no individual stocks, the weekly issue of cigarettes and food parcels occurred on a Monday. The non-monetary demand for cigarettes was great, and less elastic than the demand for food: consequently prices fluctuated weekly, falling towards Sunday night and rising sharply on Monday morning. Later, when many people held reserves, the weekly issue had no such effect, being too small a proportion of the total available. Credit allowed people with no reserves to meet their non-monetary demand over the week-end.

The general price level was affected by other factors. An influx of new prisoners, proverbially hungry, raised it. Heavy air raids in the vicinity of the camp probably increased the non-monetary demand for cigarettes and accentuated deflation. Good and bad war news certainly had its effect, and the general waves of optimism and pessimism which swept the camp were reflected in prices. Before breakfast one morning in March of this year, a rumour of the arrival of parcels and cigarettes was circulated. Within ten minutes I sold a treacle ration, for four cigarettes (hitherto offered in vain for three), and many similar deals went through. By 10 o'clock the rumour was denied, and treacle that day found no more buyers even at two cigarettes.

More interesting than changes in the general price level were changes in the price structure. Changes in the supply of a commodity, in the German ration scale or in the make-up of Red Cross parcels, would raise the price of one commodity relative to others. Tins of oatmeal, once a rare and much sought after luxury in the parcels, became a commonplace in 1943, and the price fell. In hot weather the demand for cocoa fell, and that for soap rose. A new recipe would be reflected in the price level: the discovery that raisins and sugar could be turned into an alcoholic liquor of remarkable potency reacted permanently on the dried fruit market. The invention of electric immersion heaters run-off the power points made tea, a drug on the market in Italy, a certain seller in Germany.

In August, 1944, the supplies of parcels and cigarettes were both halved. Since both sides of the equation were changed in the same degree, changes in prices were not anticipated. But this was not the case: the non-monetary demand for cigarettes was less elastic than the demand for food, and food prices fell a little. More important however were the changes in the price structure. German margarine and jam, hitherto valueless owing to adequate supplies of Canadian butter and marmalade, acquired a new value. Chocolate, popular and a certain seller, and sugar, fell. Bread rose; several standing contracts of bread for cigarettes were broken, especially when the bread ration was reduced a few weeks later.

In February, 1945, the German soldier who drove the ration waggon was found to be willing to exchange loaves of bread at the rate of one loaf for a bar of chocolate. Those in the know began selling bread and buying chocolate, by then almost unsaleable in a period of serious deflation. Bread, at about 40, fell slightly; chocolate rose from 15; the supply of bread was not enough for the two commodities to reach parity, but the tendency was unmistakable.

The substitution of German margarine for Canadian butter when parcels were halved naturally affected their relative values, margarine appreciating at the expense of butter. Similarly, two brands of dried milk, hitherto differing in quality and therefore in price by five cigarettes a tin, came together in price as the wider substitution of the cheaper raised its relative value.

Enough has been cited to show that any change in conditions affected both the general price level and the price structure. It was this latter phenomenon which wrecked our planned economy.

Paper Currency—Bully Marks

Around D-Day, food and cigarettes were plentiful, business was brisk and the camp in an optimistic mood. Consequently the Entertainments Committee felt the moment opportune to launch a restaurant, where food and hot drinks were sold while a band and variety turns performed. Earlier experiments, both public and private, had pointed the way, and the scheme was a great success. Food was bought at market prices to provide the meals and the small profits were devoted to a reserve fund and used to bribe Germans to provide grease-paints and other necessities for the camp theatre. Originally meals were sold for cigarettes but this meant that the whole scheme was vulnerable to the periodic deflationary waves, and furthermore heavy smokers were unlikely to attend much. The whole success of the scheme depended on an adequate amount of food being offered for sale in the normal manner.

To increase and facilitate trade, and to stimulate supplies and customers therefore, and secondarily to avoid the worst effects of deflation when it should come, a paper currency was organised by the Restaurant and the Shop. The Shop bought food on behalf of the Restaurant with paper notes and the paper was accepted equally with the cigarettes in the Restaurant or Shop, and passed back to the Shop to purchase more food. The Shop acted as a bank of issue. The paper money was backed 100 per cent. by food; hence its name, the Bully Mark. The BMk. was backed 100 per cent. by food: there could be no over-issues, as is permissible with a normal bank of issue, since the eventual dispersal of the camp and consequent redemption of all BMk.s was anticipated in the near future.

Originally one BMk. was worth one cigarette and for a short time both circulated freely inside and outside the Restaurant. Prices were quoted in BMk.s and cigarettes with equal freedom—and for a short time the BMk. showed signs of replacing the cigarette as currency. The BMk. was tied to food, but not to cigarettes: as it was issued against food, say 45 for a tin of milk and so on, any reduction in the BMk. prices of food would have meant that there were unbacked BMk.s in circulation. But the price of both food and BMk.s could and did fluctuate with the supply of cigarettes.

While the Restaurant flourished, the scheme was a success: the Restaurant bought heavily, all foods were saleable and prices were stable.

In August parcels and cigarettes were halved and the Camp was bombed. The Restaurant closed for a short while and sales of food became difficult. Even when the Restaurant reopened, the food and cigarette shortage became increasingly acute and people were unwilling to convert such valuable goods into paper and to hold them for luxuries like snacks and tea. Less of the right kinds of food for the Restaurant were sold, and the Shop became glutted with dried fruit, chocolate, sugar, etc., which the Restaurant could not buy. The price level and the price structure changed. The BMk. fell to four-fifths of a cigarette and eventually farther still, and it became unacceptable save in the Restaurant. There was a flight from the BMk., no longer convertible into cigarettes or popular foods. The cigarette re-established itself.

But the BMk. was sound! The Restaurant closed in the New Year with a progressive food shortage and the long evenings without lights due to intensified Allied air raids, and BMk.s could only be spent in the Coffee Bar—relict of the Restaurant—or on the few unpopular foods in the Shop, the owners of which were prepared to accept them. In the end all holders of BMk.s were paid in full, in cups of coffee or in prunes. People who had bought BMk.s for cigarettes or valuable jam or biscuits in their heyday were aggrieved that they should have stood the loss involved by their restricted choice, but they suffered no actual loss of market value.

PRICE FIXING

Along with this scheme came a determined attempt at a planned economy, at price fixing. The Medical Officer had long been anxious to control food sales, for fear of some people selling too much, to the detriment of their health. The deflationary waves and their effects on prices were inconvenient to all and would be dangerous to the Restaurant which had to carry stocks. Furthermore, unless the BMk. was convertible into cigarettes at about par it had little chance of gaining confidence and of succeeding as a currency. As has been explained, the BMk. was tied to food but could not be tied to cigarettes, which fluctuated in value. Hence, while BMk. prices of food were fixed for all time, cigarette prices of food and BMk.s varied.

The Shop, backed by the Senior British Officer, was now in a position to enforce price control both inside and outside its walls. Hitherto a standard price had been fixed for food left for sale in the shop, and prices outside were roughly in conformity with this scale, which was recommended as a " guide " to sellers, but fluctuated a good deal around it. Sales in the Shop at recommended prices were apt to be slow though a good price might be obtained : sales outside could be made more quickly at lower prices. (If sales outside were to be at higher prices, goods were withdrawn from the Shop until the recommended price rose : but the recommended price was sluggish and could not follow the market closely by reason of its very purpose, which was stability.) The Exchange and Mart notice boards came under the control of the Shop : advertisements which exceeded a 5 per cent. departure from the recommended scale were liable to be crossed out by authority : unauthorised sales were discouraged by authority and also by public opinion, strongly in favour of a just and stable price. (Recommended prices were fixed partly from market data, partly on the advice of the M.O.)

At first the recommended scale was a success : the Restaurant, a big buyer, kept prices stable around this level : opinion and the 5 per cent. tolerance helped. But when the price level fell with the August cuts and the price structure changed, the recommended scale was too rigid. Unchanged at first, as no deflation was expected, the scale was tardily lowered, but the prices of goods on the new scale remained in the same relation to one another, owing to the BMk., while on the market the price structure had changed. And the modifying influence of the Restaurant had gone. The scale was moved up and down several times, slowly following the inflationary and deflationary waves, but it was rarely adjusted to changes in the price structure. More and more advertisements were crossed off the board, and black market sales at unauthorised prices increased : eventually public opinion turned against the recommended scale and authority gave up the struggle. In the last few weeks, with unparalleled deflation, prices fell with alarming rapidity, no scales existed, and supply and demand, alone and unmellowed, determined prices.

Public Opinion

Public opinion on the subject of trading was vocal if confused and changeable, and generalisations as to its direction are difficult and dangerous. A tiny minority held that all trading was undesirable as it engendered an unsavoury atmosphere ; occasional frauds and sharp practices were cited as proof. Certain forms of trading were more generally condemned ; trade with the Germans was criticised by many. Red Cross toilet articles, which were in short supply and only issued in cases of actual need, were excluded from trade by law and opinion working in unshakable harmony. At one time, when there had been several cases of malnutrition reported among the more devoted smokers, no trade in German rations was permitted, as the victims became an additional burden on the depleted food reserves of the Hospital. But while certain activities were condemned as anti-social, trade itself was practised, and its utility appreciated, by almost everyone in the camp.

More interesting was opinion on middlemen and prices. Taken as a whole, opinion was hostile to the middleman. His function, and his hard work in bringing buyer and seller together, were ignored ; profits were not regarded as a reward for labour, but as the result of sharp practices. Despite the fact that his very existence was proof to the contrary, the middleman was held to be redundant in view of the existence of an official Shop and the Exchange and Mart. Appreciation only came his way when he was willing to advance the price of a sugar ration, or to buy goods spot and carry them against a future sale. In these cases the element of risk was obvious to all, and the convenience of the service was felt to merit some reward. Particularly unpopular was the middleman with an element of monopoly, the man who contacted the ration wagon driver, or the man who utilised his knowledge of Urdu. And middlemen as a group were blamed for reducing prices. Opinion notwithstanding, most people dealt with a middleman, whether consciously or unconsciously, at some time or another.

There was a strong feeling that everything had its " just price " in cigarettes. While the assessment of the just price, which incidentally varied between camps, was impossible of explanation, this price was nevertheless pretty closely known. It can best be defined as the price usually fetched by an article in good times when cigarettes were plentiful. The "just price" changed slowly; it was unaffected by short-term variations in supply, and while opinion might be resigned to departures from the "just price", a strong feeling of resentment persisted. A more satisfactory definition of the "just price" is impossible. Everyone knew what it was, though no one could explain why it should be so.

As soon as prices began to fall with a cigarette shortage, a clamour arose, particularly against those who held reserves and who bought at reduced prices. Sellers at cut prices were criticised and their activities referred to as the black market. In every period of dearth the explosive question of "should non-smokers receive a cigarette ration?" was discussed to profitless length. Unfortunately, it was the non-smoker, or the light smoker with his reserves, along with the hated middleman, who weathered the storm most easily.

The popularity of the price-fixing scheme, and such success as it enjoyed, were undoubtedly the result of this body of opinion. On several occasions the fall of prices was delayed by the general support given to the recommended scale. The onset of deflation was marked by a period of sluggish trade; prices stayed up but no one bought. Then prices fell on the black market, and the volume of trade revived in that quarter. Even when the recommended scale was revised, the volume of trade in the Shop would remain low. Opinion was always overruled by the hard facts of the market.

Curious arguments were advanced to justify price fixing. The recommended prices were in some way related to the calorific values of the foods offered: hence some were overvalued and never sold at these prices. One argument ran as follows:—not everyone has private cigarette parcels: thus, when prices were high and trade good in the summer of 1944, only the lucky rich could buy. This was unfair to the man with few cigarettes. When prices fell in the following winter, prices should be pegged high so that the rich, who had enjoyed life in the summer, should put many cigarettes into circulation. The fact that those who sold to the rich in the summer had also enjoyed life then, and the fact that in the winter there was always someone willing to sell at low prices were ignored. Such arguments were hotly debated each night after the approach of Allied aircraft extinguished all lights at 8 p.m. But prices moved with the supply of cigarettes, and refused to stay fixed in accordance with a theory of ethics.

CONCLUSION

The economic organisation described was both elaborate and smooth-working in the summer of 1944. Then came the August cuts and deflation. Prices fell, rallied with deliveries of cigarette parcels in September and December, and fell again. In January, 1945, supplies of Red Cross cigarettes ran out: and prices slumped still further: in February the supplies of food parcels were exhausted and the depression became a blizzard. Food, itself scarce, was almost given away in order to meet the non-monetary demand for cigarettes. Laundries ceased to operate, or worked for £s or RMk.s: food and cigarettes sold for fancy prices in £s, hitherto unheard of. The Restaurant was a memory and the BMk. a joke. The Shop was empty and the Exchange and Mart notices were full of unaccepted offers for cigarettes. Barter increased in volume, becoming a larger proportion of a smaller volume of trade. This, the first serious and prolonged food shortage in the writer's experience, caused the price structure to change again, partly because German rations were not easily divisible. A margarine ration gradually sank in value until it exchanged directly for a treacle ration. Sugar slumped sadly. Only bread retained its value. Several thousand cigarettes, the capital of the Shop, were distributed without any noticeable effect. A few fractional parcel and cigarette issues, such as one-sixth of a parcel and twelve cigarettes each, led to momentary price recoveries and feverish trade, especially when they coincided with good news from the Western Front, but the general position remained unaltered.

By April, 1945, chaos had replaced order in the economic sphere: sales were difficult, prices lacked stability. Economics has been defined as the science of distributing limited means among unlimited and competing ends. On 12th April, with the arrival of elements of the 30th U.S. Infantry Division, the ushering in of an age of plenty demonstrated the hypothesis that with infinite means economic organisation and activity would be redundant, as every want could be satisfied without effort.

Competition in Retail Trade

By W. ARTHUR LEWIS

"THE prejudices of some political writers against shopkeepers and tradesmen are altogether without foundation. So far from it being necessary either to tax them or to reduce their number, they can never be multiplied so as to hurt the public though they may be so as to hurt one another. The quantity of grocery goods, for example, which can be sold in a particular town, is limited by the demand of that town, and its neighbourhood. The capital, therefore, which can be employed in the grocery trade, cannot exceed what is sufficient to purchase that quantity. If this capital is divided between two different grocers, their competition will tend to make both of them sell cheaper than if it was in the hands of one only; and if it were divided among twenty, their competition would be just so much the greater and the chance of their combining together, in order to raise the price, just so much the less. Their competition might, perhaps, ruin some of themselves; but to take care of this is the business of the parties concerned, and may be safely left to their own discretion."

Thus spake the old master in 1776.[1] His conclusion has never been widely accepted. Retailers have always claimed that competition hurts the public no less than themselves and sought public support for their elaborate efforts to restrain it. Even the classical writers were unsure.[2] Modern economists feel more confident to tackle the problem, using as tools the newly shaped theory of monopolistic competition, but a combined knowledge of theory and of the structure of retailing is all too rare, and attempts at a synthesis all too infrequent. A further venture does not therefore seem superfluous. Neither is the moment untimely. In November, 1941, the Board of Trade made an Order prohibiting any person from opening a new retail business in this country except under licence. This was purely a wartime measure, and the President of the Board pledged himself to withdraw it after the war. But its enactment raised some hopes that permanent restrictions would follow, and recent fortunes at the polls have helped to revive these hopes. Meanwhile, some town planning authorities are already using, or planning to use, their powers in such a way as drastically to reduce the number of shops. This article may, perhaps, help to clarify the issues involved.

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[1] Smith, A.: *The Wealth of Nations* (Cannan's Edition), Vol. I, pp. 341–2.

[2] Cf. the extracts from the classical economists gathered together in Smith, H., *Retail Distribution*, Ch. IV.

The procedure adopted is to analyse first the effects of competition in prices and in services. The third section deals with certain special forms of competition which have been denounced as unfair, and the fourth section tries to reach conclusions on the main issues in the debate.

I. The Number, Size and Location of Shops

1. In the contemporary approach, the earliest doubts of the effectiveness of competition in retailing are associated with the name of Hotelling. In a stimulating article[1] he argued, on certain special assumptions, that the effect of competition would be to cause sellers to cluster together, instead of dispersing at equal distances, and showed that this undesirable result in location causes transport costs to be excessive. Various writers interested themselves in the problem, but the outstanding advance is the contribution of Lerner and Singer eight years later, in an article[2] which generalises Hotelling's case, and is the most convenient starting point.[3]

The basic assumption is that the customers are strung out at equal distances along a road. Then, on the further assumptions (*a*) that no two shops can be on the same spot ; (*b*) that each end of the road is a cul-de-sac ; (*c*) that each customer pays his own transport costs ; (*d*) that the number of shops is given ; (*e*) that the price is fixed ; and (*f*) that there are no economies of scale, Lerner and Singer show that in equilibrium (i) there cannot be more than two shops together, and (ii) there must be two shops together at each end of the road. They also imply that each shop must have the same number of customers, unless there is an odd number of shops, when one may have more than the others ; but this is not so. The third condition of equilibrium is only that no shop may have less customers than half the number between any two other shops, or less than the shops at the end. Shops may be equidistant, but need not be, and as in any case the end shops must be paired, transport costs must be above the ideal.

Removing some of these assumptions brings the equilibrium conditions nearer to the ideal. Assumption (*a*) makes a negligible difference. Removing assumption (*b*) destroys conclusion (ii) ; if the road connects say two big market centres, the first shop at either end of the road may stand alone. As for assumption (*c*), Lerner and Singer themselves reach the important conclusion that if transport costs are paid not by the customers but by the sellers, shops will be located at equal distances from each other, which is the ideal situation.

2. To advance the analysis beyond the Lerner-Singer stage it is necessary to remove the remaining assumptions. If the number of

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[1] Hotelling, H.: "Stability in Competition", *Economic Journal*, June, 1929.

[2] Lerner, A. P., and Singer, H. W.: "Some Notes on Duopoly and Spatial Competition", *Journal of Political Economy*, April, 1937.

[3] From the bibliography included in the latest edition of Chamberlin, E. H.: *Theory of Monopolistic Competition*, a number of articles bearing on this subject seem to have appeared in American journals since 1939. On account of the war these journals have not yet arrived at the Library of the London School of Economics, and I have not been able to consult them.

shops is not given, and prices are not fixed, and there are no economies of scale, the number of shops will be as great as the number of customers. This odd conclusion brings into the open the odd assumption underlying the whole analysis that transport cost from wholesaler to retailer can be neglected, but not transport cost from retailer to customer, an assumption which becomes the more unreasonable the greater the ratio of shops to customers. Nevertheless, the conclusion serves to remind us that the convenience of customers requires that there should be as many shops as possible, each small, and that it is only economies of scale which prevent this. The assumption of constant costs is incompatible with equilibrium in retailing, given dispersal of customers. For if a reduction in the size of shops will not increase costs, it will pay some seller to insert himself between two existing shops.

Should shops, then, be of " optimum " size, meaning the minimum size consistent with minimum average cost, or should they be smaller, and if so, how much smaller ? If we can assume that the inconvenience of not having a shop nearby can be translated into monetary terms as a function of the distance of the shop, the problem is capable of precise solution.

Let us assume (1) that all shops have the same costs ; (2) that there are no prime costs, but only an overhead, so that the average cost curve is a rectangular hyperbola (marginal costs are assumed away only for convenience of exposition ; what matters is the assumption of falling average cost) ; (3) that the customers are strung out along a road on either side of each shop, one customer for each unit of distance ; (4) that each customer buys one unit (money value) of merchandise ; and (5) that the inconvenience of distance can be expressed as one unit of money per unit of distance, paid by the customer. Then the following diagram shows the position.

AC is a rectangular hyperbola, representing the average cost curve of any shop, varying with the number of its customers (and therefore sales). *MTC* is the curve of marginal transport costs ; the first two customers, one on either side, incur one unit each, the next two customers incur two units each, and so on ; the curve can thus be drawn for simplicity as a straight line with a slope of 0·5. Total transport cost is the area lying beneath it, and average transport cost per customer will be a line with a slope of 0·25.

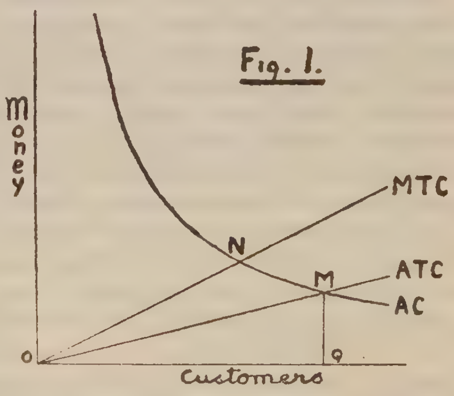

Fig. 1.

The ideal number of shops (or number of customers per shop) is such that, given the total number of customers to be served, the sum of shop costs and transport costs is at its minimum. This condition is fulfilled when for each shop the sum of average shop costs and average transport costs is minimised. Since for each new customer the former falls and the latter rises, the condition is fulfilled when the negative slope of AC equals the positive slope of ATC. AC being a rectangular hyperbola and ATC a straight line from the origin, this will be at the point where they meet. M gives us the ideal size of shop. It cannot correspond with the point where average shop cost would be minimised; since ATC is rising and AC is falling, the minimum point of $ATC + AC$ must be to the left of the minimum point of AC. In other words, it is desirable that shops should be of less than "optimum" size.

Will competition bring about the right size? If there is free entry, and if there cannot be abnormal profits, the price charged by each shop (its gross margin) must lie somewhere along AC. If a shop reduces its price by one unit it will attract two customers, one on either side, who transfer their custom from its rivals. It pays therefore to lower the price by one unit if getting two more customers will reduce AC by more than one unit. Its equilibrium is therefore at the point N where the slope of MTC equals the slope of AC.[1] Thus shops will not cut their prices low enough to bring the right size. In competition, there will be too many shops, each too small.

The result can be generalised by assuming that consumers do not live along a road, but are distributed over an area, one in each unit of space. It remains desirable that shops should be equidistant, and this condition can only be fulfilled if each is at the centre of a hexagon. How many shops there should be, and what should be the size of each, is then determined if we know how large the hexagon should be. Retaining our previous assumptions, the solution is as follows. If we write r for the length of the radius of the inscribed circle, the length of each side will be $1{\cdot}15r$. The number of customers equals the area of the hexagon, $3{\cdot}46r^2$. Writing a for the overhead costs of each shop, average cost per customer,

$$AC = \frac{a}{3{\cdot}46r^2}.$$

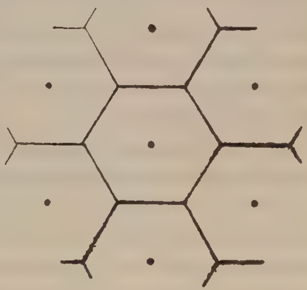

Fig. II.

To calculate transport costs we first divide the hexagon into its twelve right-angled triangles. In each, taking the customers living along the base, the nearest lives at distance r and the furthest at distance

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[1] N corresponds to Chamberlin's "ideal adjustment" and to R in Fig. 15 of his *Theory of Monopolistic Competition*, p. 92. The slope of MTC here corresponds to the slope of dd^1 in that diagram.

$1{\cdot}15r$. The sum[1] of the distances for the base customers is $0{\cdot}61r^2$, and the total transport cost for the hexagon is twelve times the integral of this, i.e., $2{\cdot}41r^3$. This divided by the number of customers gives average transport cost per customer,

$$ATC = 0{\cdot}70r.$$

The ideal number of shops is given by the condition that the slopes of *AC* and *ATC* must be the same. Differentiating gives

$$\frac{a}{1{\cdot}73r^3} = 0{\cdot}70$$

$$\therefore\ r = 0{\cdot}93a^{\frac{1}{3}}.$$

Now, in competition a shop will cut its price by one unit so long as the resulting increase of customers reduces *AC* by more than one unit. Equilibrium therefore results when

$$\frac{a}{1{\cdot}73r^3} = 1$$

$$\therefore\ r = 0{\cdot}83a^{\frac{1}{3}}.$$

This analysis brings out the relevant points. The number of shops depends on the extent of the economies of scale, the density of population (determining the number of customers per unit of distance), and the inconvenience or cost of distant shopping. The greater the economies of scale, and the less the cost of transport, the fewer shops there will and should be. The denser the population, the larger shops will be, and this is worth noting since many people believe that the decentralisation of urban populations which has occurred since 1920 is one of the principal factors explaining the increase in the numbers engaged in distribution in this country.[2] In any case, given the dispersion of customers, the ideal size of shop is less than the "optimum"; and, given competition, the actual size will be less than the ideal.[3]

3. Several qualifications are needed before we can derive from this anything like a complete picture.

First, the analysis has so far assumed that there is price competition, and that each shop cuts prices without taking the reaction of its competitors into account. This does not require that the price of every one of the hundreds of commodities it sells should be keenly competitive; it is enough that average gross margins for any class of trade must be adjusted to the level set by competition, and if this is so, the effect on size is the same. But even this is not a complete picture. To begin with, retail prices in certain trades—cigarettes, patent medicines, some groceries, milk, coal, confectionery, books, periodicals, stationery, lamps, cycles, petrol, motors and motor accessories—are maintained by elaborate arrangements between manufacturers, wholesalers and retailers, which impose heavy penalties for price cutting. The assumption that retailers are free to compete in prices however holds good in other trades—some groceries, fish, meat, fruit and vegetables, flowers, furniture, most hardware, drapery, footwear, clothes, pottery and others—and to these the analysis applies, subject to further qualifications.

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[1] I am indebted to Professor R. G. D. Allen for the formula.

[2] E.g., Ford, P.: "Changes in the Number of Shops, 1901–1931", *Economic Journal*, June, 1936.

[3] The above example assumed that goods are transported as the crow flies. More realistic assumptions do not upset the argument unless immensely complicated routes have to be used. For example, if we assume that main roads run direct from shop to shop, and that side roads are perpendicular to these main roads at intervals of one unit, total transport cost becomes $2.98r^3$; the optimum number of shops will be given by the equation $r = 0.88a^{\frac{1}{3}}$, and the competitive number remains at $r = 0.83a^{\frac{1}{3}}$.

When prices are fixed the number of shops is a direct function of the level at which they are fixed. In terms of Figure III (which is constructed on the same cost assumptions as Figure I), equilibrium is given by the point *K* at which the horizontal price line *SP* cuts *AC*. So long as shops are larger than this they will be making profits; new entrants will then be attracted, reducing the share of each in the trade, until abnormal profits vanish. As the price is usually too high under these schemes (for reasons we shall examine later) the number of shops will be excessive. At this point, however, it will pay any one shop to compete by offering better service. If all shops do this, as they must in competition, all make losses unless their number contracts, and in competition it will contract up to the point where the new cost curve (including the additional services) has the same slope as *MTC*. If, as in Figure III, the extra services take the form of an addition to overhead costs, shops will be larger than they would be if there were competition in price but not in service, but prices will be higher. If they take the form of an addition to marginal costs, shops may be larger or smaller, high or rising marginal costs making for smaller shops.

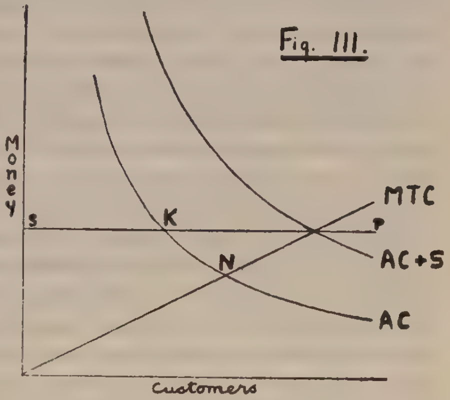
Fig. III.

Shops may, of course, compete neither in prices nor in service, even though prices are not maintained. If each shopkeeper takes into account the fact that if he cuts prices or offers more service his rivals will follow, shops will be still smaller and more numerous. The limit to their smallness will be set by the elasticity of demand in the market as a whole, i.e., by *Q* in Chamberlin's Figure 15[1]; in our diagram where elasticity is assumed to be zero it would be one shop for each customer. It is most unlikely in practice that this movement towards the upper limit will get very far. It is certainly unlikely in the United States of America, where aggressive competition is the rule, with chains, super-markets and independents at each other's throats. Competition is not so keen in this country, but competition between the co-operatives, the multiples and the independents is nevertheless keen enough in non-price-maintained trades to keep the size of shops near to N. In the price-maintained trades price competition is ruled out; service competition tends to check the decrease in shop size, but it is not always very strong and, given the fall in wholesale prices which had been occurring steadily after 1920, it was possible for more and more redundant shops to establish themselves on an ever widening gross margin.

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[1] Loc. cit., p. 92.

4. The second qualification concerns transport costs. The conclusion that in competition there must be too many shops depends on the assumption that customers pay transport costs. If the shop pays transport costs, it will take on an extra customer so long as the price exceeds the cost of transporting to him (marginal selling cost being on our assumptions zero). But since (Fig. I) MTC equals the price $(AC + ATC)$ at the point where $AC + ATC$ is at its minimum, it follows that the point of equilibrium for the shop paying transport costs is the same as the ideal equilibrium, represented by the output OQ. There will be too many shops if customers pay their own transport costs, but the ideal number of shops if there is free delivery.

The reason for this is that free delivery is a form of price discrimination (in this case perfect discrimination), and, if average costs are falling, discrimination usually brings output nearer to the ideal. The discrimination arises because the nearest customers pay a price in excess of AC plus the cost of transporting to them, and the furthest pay the same price which is much below AC plus the cost of transporting to them (indeed the furthest customer contributes nothing towards AC); in a sense the nearer subsidise the further. The same sort of result would be attained by any other form of discrimination, and is not the result of competition in service as such. If all the customers get as much service as they pay for, the result is merely to lift the average cost curve; shops may be larger, but the gap between the ideal and the competitive equilibrium is not diminished. However, there are also forms of service other than delivery which involve discrimination, e.g., credit and subsidised restaurants in department stores. Whenever a service is paid for by some who do not use it, and used by some *who would buy elsewhere but for this service*, the effect is to bring the competitive equilibrium nearer to the ideal equilibrium.

We must also note precisely what transport cost means in this context. MTC is a measure of all the inconvenience associated with buying at a distant shop. It is not the same as what it would cost the shop in money to deliver one's purchases. A customer who shops in the centre of the town may be quite capable of carrying home a pair of shoes without much extra trouble or inconvenience, and if asked to pay an extra sum equal to what delivery would cost the shop, might prefer to carry for himself. When therefore one concludes that shops will be larger and of optimum size if they offer free delivery but not otherwise, one concludes too much. If the firm's transport costs exceed the value of the real convenience conferred on the customer, the shop will not reach optimum size even if there is free delivery, and may indeed be smaller than it would be without free delivery if the disparity is great.

5. The third qualification is to remove the assumption of zero elasticity of demand. It will be more profitable for the shopkeeper to lower his price if he can count on selling more to his existing customers as well as on attracting other people's customers, so an elasticity exceeding zero will bring the competitive number of shops nearer to the ideal number, or even below it. There may be no "excess capacity" if elasticity is large enough. Of the many modifications elasticity introduces into the results, this is the important one.

6. Next we must consider the assumption that there cannot be abnormal profits in equilibrium. This rests on two further assumptions. The first is that customers have no irrational preferences for shops, so that if two shops differ in nothing but their average price levels, customers will always buy from the cheaper. There has been some attempt to explain retailing, using as a large element in the analysis the irrationality, ignorance and inertia of customers.[1] While it is impossible to deny that these factors have some importance, it is impossible to agree to any significant importance in view of the rapid changes constantly taking place in the structure of retailing, as reflected in the relative strengths of independents, multiples, department stores, street markets, and the co-operative societies. Any shopkeeper who used the irrationality, ignorance, or inertia of his customers as major elements in his policy would soon find himself in Carey Street.

The other assumption necessary to keep prices equal to average cost is that continuous variability in the number of customers per shop is possible in each locality. This cannot be taken for granted. In the suburbs a new shop sets up between two other shops, and its impact is borne more heavily by the sales of these shops than it is by the sales of other shops. At any given moment, therefore, with a given number of shops, it may be that if a new shop could be introduced in such a way as to take trade away equally from all shops (i.e., if all the shops were re-located at shorter intervals and one added), the new shop could remain permanently, but that since it would in fact be limited to sharing in the trade of two or three others, the shop will not be established. Certain consequences flow. First, profits may be abnormal without new shops being established, because there is more than enough for two but not enough for three ; the corollary is that shops may be larger and fewer than the true competitive equilibrium. On the other hand, if too many shops come into existence, there is less likelihood of price or service competition, because in a small market sellers are more likely to take account of each other's reactions ; and the entry of a new shop results simply in higher prices. There is, however, a check on this. Suburban shops compete not only with each other but also with shops in the large shopping centres, which are more aggressive. This fact makes possible continuous variability ; it prevents suburban shopkeepers from exploiting a local monopoly, and keeps down the number of shops.

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[1] See especially Smith, H.: op. cit., Ch. V.

7. The fifth qualification is now to take full account of the existence of these large shopping centres.

From the analytical point of view, the difference is that we remove the assumptions that the customers are strung out evenly, one in each spot, and that transport costs are involved. For at a shopping centre there are thousands of persons who find themselves there for reasons other than shopping, and who, once there, can shop without counting transport cost. It we remove the assumption of transport costs, and assume large enough numbers of customers, it follows that in a large shopping centre shops should be of " optimum " size, meaning that average cost is at a minimum. The customer in Oxford Street takes real feminine pleasure in shopping ; she moves around comparing prices and qualities, consulting her friends, and taking hours (or days) to make up her mind. It would be too much to claim that elasticity of demand is infinite for each shop, especially in the naïve sense that a small shaving of price immediately expands sales ; but it is not too much to say that, in trades where the customer shops around, shops with cheap prices will sooner or later bankrupt those which try to sell similar qualities with similar service at a higher price level in the same centre. In the long run, therefore, shops in large shopping centres must approximate to optimum size.

This conclusion at once explains two common phenomena. First that shops in the centre are usually larger than those in the suburbs, and secondly that their prices are in consequence frequently lower[1] ; and it reinforces the point that the suburban shop has only a precarious local monopoly. At the same time it raises another question. Since there is usually more than one shop of each kind in a large shopping centre, an " approximate long run infinite elasticity of demand " is consistent only with the assumption that the economies of scale are fairly easily exhausted. Otherwise there would be only one shop of each kind. How true is this ?

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[1] Differences in rent complicate the issue. Higher rents may or may not outweigh larger turnover. For department stores of equal size, rent is decisive in the United States; see Burnham, E. A.: " The Department Store and its Community ", *Harvard Business Review*, XVIII, p. 455. It does not seem to be so decisive in England, but the evidence is not so clear; see Plant, A. and Fowler, R. H.: " Costs of Retail Distribution ", ECONOMICA, May, 1939. For some theoretical discussion, see Chamberlin, E. H.: loc. cit., Appendix C.

The evidence is scanty, contradictory, and difficult to assess. It is quite clear that a medium-sized shop has lower costs than a very small one-man shop, if similar factors are similarly remunerated. But, of course, this is not always so. The small man who converts his parlour into a shop where he or his family serve part-time may have very low transfer costs, and be able to carry on indefinitely with low gross margins. To return, however, it is clear that where transfer earnings are similar the medium-sized shop has lower costs than the smallest shops. For example, Colin Clark's analysis[1] of the grocery trade of a number of stores controlled by a large co-operative society revealed the following situation :—

Average turnover, £ per week	Costs (including interest but excluding profit) as percentage of turnover
100—200	13·21
200—300	11·51
300—400	11·13
400—500	10·50
500 upwards	10·08

A similar picture is suggested by Cadbury Brothers as applying to confectionery shops.[2]

It is in comparing the medium and the large shop that the evidence is inconclusive. The authors of a careful American survey[3] conclude that, apart from the high cost of the one-man business, there is " no convincing evidence of a general tendency for costs to decrease progressively as store size increases ", and proceed to quote exceptions. Everything depends on the nature of the trade. The principal economy of large shopkeeping is the economy of having specialist buyers purchasing in large quantities. The more the goods can be standardised the greater is this economy. It is therefore small in the millinery business, but large in the Woolworth type of trade. The classes of shop which most multiply themselves in central shopping centres, those selling women's wear and household goods are the classes in which standardisation tends to be least, and the advantages of scale small. The shops selling standardised goods are seldom found in great numbers even in the largest shopping centres, because it is more convenient for them to be widely distributed through the suburbs. They secure the economies of large buying through concentrating several shops under one management rather than by having a large turnover per shop ; but the number of customers congregating in the centre may be sufficient to support two or three or more in any trade, each of more or less " optimum " size.

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[1] Clark, C. : *Conditions of Economic Progress*, p. 334.

[2] *Industrial Record*, 1919—1939, p. 49.

[3] Twentieth Century Fund, *Does Distribution Cost Too Much?* p. 145.

The department store fits with difficulty into the pattern of this argument. In a large shopping centre there is usually more than one. Yet the evidence is clear that there are substantial economies of scale, both in the United States[1] and in this country.[2] It is true that the American evidence shows the medium-sized department store having lowest costs, but this is because delivery and publicity expenses are higher for the largest stores, and these expenses are not relevant if we are considering merely competition between two stores in the same centre, with nearly infinite long-run cross elasticities. The large department store should be able to drive all others out of its centre simply by cutting prices. Why, also, does it not drive out the specialist shops?

The latter is easier to answer. It must be remembered that even the largest department store is really only a collection of several small or medium-sized shops under one roof. A large department store may be able to buy more cheaply than a small one. But the shoe department of a large department store is not necessarily bigger than an independent shoe shop, and not necessarily able to buy more cheaply. Indeed, department stores are keenly conscious of the competition of the specialist shops. This is one reason why the multiple shop idea has spread over into the department store field, and why some of these multiple department stores have now adopted central buying. But the main strength of the department store is not price competition, but the fact that it offers the opportunity to buy everything under one roof, plus delivery, credit, and various amenities, and on these it concentrates its appeal. It is really doing a different class of trade from the specialist shop, and not strictly comparable.

This also yields the clue to competition between department stores themselves. The price competition is real, and governed also by the need to compete with specialist shops both at the centre and in the suburbs, which may be able to offer lower prices because they offer also less service. But because service is so important, price competition is not decisive. No two department stores have quite the same atmosphere, or give quite the same service, and the larger would not necessarily bankrupt the smaller by cutting its prices 5 per cent. Moreover, even if it could, the struggle would be long and costly. Prudence, and the small number of department stores, demand that while price competition should be real, it should be " based on costs ", within the limits of what competition by specialist shops allows. In effect, the department store trade is rather special, and though it has close substitutes, is sufficiently limited for the oligopolistic phenomenon of " excess capacity " to prevail even in the shopping centre.

Moreover, in trying to analyse the existing situation, we must not treat it as if it were in " equilibrium ". The department store in the 1930's was going through an awkward phase. When first it appeared, in the second half of the nineteenth century, it was a pioneer of modern methods of retailing, and its wide success was largely due to outdistancing its rivals in this respect. By the 1920's its rivals had caught up with it, and were giving it a stiff fight, and it is doubtful whether it continued to expand relatively to other forms of retail trade. It has been forced to review its policies, to decentralise internal management, to consider central buying with other stores, and to rely more and more on service. As costs of service have risen, the policy of relying on service has itself been brought into question, since some competitors, unhampered by the high costs of free delivery, rest rooms, subsidised cafés, credit, and the rest, have pushed the price feature in their " cash and carry " policy. Most of the department stores in the United States and many in this country were losing money during the nineteen thirties. It would be unwise to prophesy what policies they will be pursuing in the fifties.

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[1] Cf., Burnham, E. A.: " The Influence of Size of Business on Department Store Operating Results ", *Harvard Business Review*, XVI, p. 211.

[2] Cf. Plant, A. and Fowler, R. F.: loc. cit.

8. We have thus two different types of analysis to apply to retailing, one assuming that the customers are dispersed and find a dispersal of shops convenient (with an excessive number[1] of small shops as the result), and the other assuming that the customers are concentrated in shopping centres (with the right number of shops, of optimum size). Both, of course, apply ; moreover to some extent they apply to quite different classes of trade. Here the American distinction between " convenience " and " shopping " goods is useful. The former are articles purchased regularly and preferably from the nearest shop. The customer knows from daily experience just what he wants : there is no " shopping around ", and the shops tend to be spread evenly over the whole area—beef, fish, cigarettes, groceries, beer, medicines and the like tend to be bought at the nearest outlet. With shopping goods, on the other hand, the customer wishes to compare prices and qualities before she makes up her mind—hats, curtains, dresses, even stockings —and the shops selling these things tend to congregate in shopping centres. The division is not watertight ; some convenience goods shops of more or less optimum size are to be found in shopping centres, in competition with the suburban shops ; while it is possible to find shops selling shopping goods in isolation in the suburbs, e.g., the ubiquitous draper. Department stores are in both classes of trade.

One further point is needed to complete the picture. So far we have spoken as if shops are to be found either clustered together in a central area, or spread over the suburbs in lonely isolation. A little observation shows that nearly all shops are in a centre of some kind. Nevertheless the distinction between the central and the suburban shopping centres remains. The shops which are found together in the suburban shopping area tend to be shops selling different classes of goods. These shops are not in close competition with each other, except in the sense in which all shops are competing for the consumers' incomes, and the fact that these different shops stand together does not invalidate the analysis. The theory of the optimum number of suburban shops becomes instead a theory of the optimum number of suburban shopping centres. Account must, however, be taken of the fact that these centres vary in size. This is due to the fact that the desirable frequency of shops varies in different trades, with the importance of overhead costs, the convenience of nearness, and the percentage of the population consuming the particular commodity. We should expect to find a large number of small centres, and smaller numbers of larger ones. Precisely where each centre is and which centres are large, are determined more by geographical considerations—the layout of streets, the junctions of main roads, the flow of traffic, and so on—than by the differential calculus, but overhead cost, density and convenience, the factors included in our calculations, are plainly not irrelevant. Ranging shopping centres in order of size, from the smallest to the largest, we should find that some suburban centres have sufficient trade to support several shops of one kind, each of optimum size, while in other trades there may be only one shop, of less than optimum size, and in others no shop at all. From the point of view of the trades in the first category this is a central shopping centre, because it is large enough to support more than one shop of optimum size, while from the point of view of the trades in the other categories it is a suburban shopping centre because it is not large enough. As the size of centres increases, so does the number of trades in which shops can be of optimum size, and to which this is a central shopping centre, until we reach the largest centres, with their department stores straining to reach the optimum.

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[1] Unless elasticity of demand is large enough to bridge the gap.

9. We may now return to the end of section 6, where we began to take account of the effect of the existence of central shopping centres on shops in the suburbs. The first effect is a tendency for shops to be both smaller and at greater distances than they would be from each other, because the density of buying population in the suburbs is reduced. On the other hand, suburban shopkeepers, having to face the competition of central shopping areas, are less likely to try to exploit local monopoly positions, and this increase in competitiveness is a factor on the side of lower prices and fewer and larger shops. The extent of price competition, however, varies. In price-maintained convenience goods trades—and nearly all such trades are price-maintained—the suburban shopkeeper faces only competition in service, which, at a distance, may not be very effective. In such trades it is certain that there will be too many shops. In the shopping goods trades the position is different; the suburban shops are faced with both price and service competition from shops of optimum size at the centre, and are compelled to keep their prices as low as is consistent with the fact that they themselves cannot attain to optimum size.

(This is one of the reasons why price maintenance is unlikely to spread over into such trades.)

10. We may conclude by summarising :—

(*a*) Suburban shops should be of less than optimum size ; if there is free entry into retailing they will be smaller and more numerous than they should be, unless elasticity of demand is large enough to bridge the gap ; and if there is price maintenance they will be still smaller and more numerous, with perhaps too much service.

(*b*) Free delivery and other discriminatory services tend to bring shops nearer the ideal size and number.

(*c*) In central shopping centres shops will be of optimum size, and shops selling shopping goods will tend to cluster together here ; department stores are an exception in that they are probably too numerous and too small.

II. COMPETITION IN SERVICE

1. Retailing is not a homogeneous "commodity" ; it varies widely in quality, and correspondingly in cost. We must now examine the contention that the effect of competition is an excess of the more expensive forms, as well as the reverse contention, that competition causes the quality of the service to deteriorate.

It is useful to begin by distinguishing the features which make for expense. One is the very existence of service. "Help yourself" shops exist in the United States, and show the lowest margins of all. Then there is the speed with which service is effected—how long the customer has to wait before an assistant attends to him. Speedy service is particularly costly because the flow of customers into the shop is irregular. If the shop tried to keep enough assistants to be able to deal with the largest number of customers ever likely to be in the shop, most of these would be idle for much of the time, sales per assistant low and operating costs high. In retailing as in transport, costs would be much reduced if customers could distribute themselves evenly throughout the day and (though this is not so important) throughout the year. Next comes the quality of the assistants. In some trades the customer relies on the shopkeeper for information on the comparative qualities of the merchandise, and shop assistants have to be specialists trained in its qualities and uses. This means not only a more expensive kind of assistant, but also a smaller average number of customers per assistant than in, for example, the Woolworth type of trade. Another important service is performed when the shop offers the customer a wide choice of styles, colours, sizes or makes from which to suit his requirements. Some shops, especially department stores, add to this the right to return the purchase if on going home the customer changes her mind—American department stores report that one day's sales in eight are returned[1]—and find this a heavy burden. Lifts, escalators, rest rooms, credit, free delivery, information —the list becomes too long for elaboration.

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[1] Twentieth Century Fund, op. cit., p. 308.

Account must also be taken of forms of retailing which do not involve the customer in going to the shop. Orders by telephone or by mail are convenient if the customer knows just what he wants, and this form of business has low operating costs. Or the shop may go to the customer, in the form of the travelling van or the door-to-door salesman. Here operating costs are usually high.

2. In order to arrive at the conclusion that competition leads to an excess of the more expensive forms of retailing, in substitution for the less expensive, it is necessary to establish one of two propositions —either that customers get what they want, but should not be allowed to get it; or that they get what they do not want, through market imperfection. The first proposition has its adherents; they admit that the customer prefers good service even when there is a real choice open to him of less expensive forms, but they contend that this is a luxury he should not be allowed. In wartime we have all accepted this view in the United Kingdom, in order to release resources for other purposes. In peacetime too, "productive" output would be greater if "distributive" activity were curtailed, and in a planned economy a planning authority which attached little value to convenience in shopping relatively to increased outputs of other utilities would undoubtedly take steps to restrict the more expensive forms of retailing. In this country, however, where women exercise the vote, such restrictions are unlikely. To a busy woman—and with the decline of domestic service and the increased entry of women into jobs and professions nearly all women will be busy women—good shopping facilities are essential; if she can get served without waiting, see at once a wide range from which to choose, and obtain delivery, she can get back rapidly to her duties; to reduce such facilities would mean that the public spent more time in shopping, and some of what was gained in distribution costs would be lost in other ways.

It is more important to examine the proposition that some who would prefer less service and lower prices are deprived of it. This depends on market imperfection. In a large shopping centre where all kinds of shops are competing it is highly unlikely. There are shops with much service and high prices, and shops with less service and lower prices, and the customers are free to sort themselves out. In 1931 one of America's leading authorities on retailing[1] wrote:—

> "To judge from what is happening in the department store field and from what is happening in the chain store field, it is a fair generalisation that types of distributive enterprise tend to develop through three stages. They start off very largely on a price basis—as chain stores did for instance, they catch the attention of the consumer by distributing merchandise at low prices because of a low overhead. That is the first stage. The next stage is the 'trading up' of the quality of the merchandise handled. We can see chain stores going through that stage to-day. After

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[1] McNair, M. P.: "Trends in Large Scale Retailing", *Harvard Business Review*, X, p. 39.

> they have traded up the quality of the merchandise handled, and some of the price advantage has been lost in the process, distributive enterprises develop into a third stage, characterised by competition in services of all kinds, for instance in allowing customers to return merchandise; by high costs of doing business; by largely competitive advertising; and by an increase in the ratio of the fixed investment to the total investment. Department stores to-day are in that stage."

This would seem to support the view that the public inevitably prefers better services to low prices, but events have not justified this conclusion. The department stores, having reached the third stage, have found themselves losing so much to cheaper rivals (including the "super-market", which was just making its appearance when the passage was written) that they have greatly extended their "bargain basements", and some of them are beginning to urge on the fraternity that delivery and similar services should be charged for separately, so that the stores can retain some of the custom they have been losing to cheaper rivals. The tendency to higher costs, if it exists, is only like an escalator in the sense that as fast as some move up others come in at the bottom; the better simile would be a cycle, since the fourth stage brings lower costs than the third. The fact that the public is free to choose between the cheaper and the more expensive acts as a check preventing too much service from being offered.

The practice of "price lining" may seem to support the opposite view. If prices tend to stick around certain figures, e.g., the eleven pence three-farthingses, increased efficiency would seem to lead not to lower prices but to better quality at the same price. This, however, is an illusion, since this better quality now sold cheaply was formerly sold at some higher price level. Price lining prevents prices from being continuously variable, but it does not prevent them from falling.

However, we cannot apply the same analysis to shops situated not in central but in local markets. There the element of local monopoly means that the customer has not an effective choice between several shops offering different degrees of service. Here there may well be too much service, especially in price-maintained trades, in the sense that some customers are getting more than they want. In non-price-maintained trades the competitive incentive is not so much to offer service as such, if the shop is able to offer different customers different degrees of service, as to offer types of service which involve price discrimination in favour of marginal customers, and which tend therefore to bring the shop nearer to the ideal size. Is this desirable, or does it mean that there is too much service? There are two sides to the argument. Clearly there is too much service in the sense that the marginal customers are getting more than they pay for: they are being subsidised. On the other hand, since the shop will have more customers, shops will be larger, fewer in number, and less costly. It is the old argument for price discrimination in new form.

If the services offered are real, and performed at minimum cost, we can say that there will not be too much service even in imperfect competition, provided that the imperfection is not due to any significant extent to "irrational" elements. The three services which most merit analysis in this light are information, a range of styles, and delivery.

3. Information is a real service. We are all conscious of the wastes involved in competitive advertising, and tend to dismiss all publicity expenditure as wasteful. That it is not necessarily so has long been emphasised in economic literature.[1] For a market to be perfect all the buyers and all the sellers must know what is available and at what prices. Publicity expenditure which serves this purpose is really useful. Advertising is wasteful only when it is unnecessary because buyers and sellers already know the facts, or when it is false or misleading. Now, while much advertising by manufacturers is wasteful for these reasons, this is not usually the case with retailers' advertisements. Actually, only a very small proportion of retailers think it worth while to advertise, this itself being a tribute to the fact that the retail market is not as imperfect as some writers have suggested. And perusal of the advertisements published by retailers will show that while the element of reputation building is not absent, they tend to concentrate on straightforward information about particular lines which are on offer. It seems therefore legitimate to conclude that in retailing such publicity expenditure as there is performs for the most part a valuable service.

We cannot, however, dismiss manufacturers' advertisements as irrelevant to competition in retailing, for they are not. Since there is competition, every retailer is subject to pressures requiring him to stock as wide a range as his customers are likely to want to choose from, and this is indeed a factor on which manufacturers strongly rely in spending money on advertisements. This, therefore, brings us straight to the second problem, the range of goods retailers carry.

4. If in each commodity demand concentrated on one or two styles, economies of scale could be exploited, and prices would be lower. This is the basic case for the "utility" scheme in wartime England. The first question is therefore why the market itself does not provide for getting these economies. To some extent it does. If a retailer finds that by concentrating all his efforts on a single style he can buy and sell more cheaply, he has an incentive to do so. The Woolworth type of business, which is one of the most successful forms of retailing, is based on this principle ; the multiple shops, the department stores, and many independents also work on this basis to a larger extent than is generally recognised by those who advocate that the "utility" scheme should be made permanent. Broadly speaking, standardisation is the basis of large-scale retailing, and one of the reasons why in

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[1] E.g., Braithwaite, D.: "The Economic Effects of Advertising", *Economic Journal*, March, 1928.

so many lines of production the retailer dominates the manufacturer.[1] In small-scale retailing, the retailer is not able to give large orders in standard designs, but relies rather on the wholesaler to do this. The wholesaler is to the small retailer as the centralised buying department is to the multiple shop, and if wholesalers were as efficient as the buyers of large-scale retailing, the whole pattern of retailing would be different. When Mr. Schumacher suggests virtually a state monopoly of wholesaling as the simplest way to control both production and distribution,[2] he certainly puts his finger on the central weakness in the distributive chain (whatever the merits of the particular proposal). More efficient wholesaling would deprive the larger retailers of their principal advantage over the small, and would promote greater standardisation of manufactures.

But, whatever the desire of large retail buyers or wholesalers to standardise, two other forces act as a check. One is the creation of "consumer insistence" by manufacturers' advertisements. The case made for such advertising is varied. (*a*) Some useful commodities, e.g., the typewriter, would never have reached the public as quickly without it. This is undoubtedly true; some advertising performs the useful function of information. But this does not apply to all the £80,000,000 to £100,000,000 a year spent on advertising in this country.[3] (*b*) Advertising frequently leads not to an increase but to a reduction in the number of styles, and to savings in production cost, by persuading the public to concentrate most of its purchases on one or two well advertised brands. This also is true. But the opposite is also frequently true; and if from the £100,000,000 we must subtract something for the cases where economies of scale are secured, if we are to have the true cost of advertising, we must also add something for the cases where consumer insistence hinders standardisation. Mr. Bishop also advances two other ingenious arguments. (*c*) Advertising makes people want things they cannot afford; it therefore increases the propensity to consume and consequently increases employment. The argument might be impressive if there were not less costly ways of maintaining employment, but as there are, economists cannot be expected to take the argument seriously. (*d*) The press is always subsidised; where proprietors cannot get revenue from advertisers they sell themselves to political parties or the government, and in the long run subsidy by advertisement is far more healthy for the public. This may be true. But there are alternatives. If the government decided to prohibit advertising in the press, and if the press must be subsidised, let the government undertake to pay a subsidy varying only with circulation; e.g., to pay newspaper proprietors 1d. for each copy sold, on the understanding that there shall be no advertisements. Provided the subsidy were open to all on these terms, the press would remain independent and free to criticise. It would in fact be much freer than it is to-day, when the political and other views of large advertisers have to be borne in mind.

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[1] Nearly all the important groups of multiple shops in this country are manufacturer chains, in contradistinction to the United States.

[2] Schumacher, E. F.: "An Essay on State Control", *Agenda*, February, 1944.

[3] This is the estimate made by Bishop, F. P.: *The Economics of Advertising*, Ch. III.

Some reformers are content to try to control the impact of advertising. There are some who advocate strict legal controls, penalising misrepresentation, as the Federal Trade Commission seeks to do in the United States.[1] Others hope that in this country, as in the United States, independent testing agencies will grow up, private- or State-maintained, to inform the consumer of the true merits of different brands. Or even that the press will allow manufacturers to speak plainly in their advertisements about the inferiority of their rivals' products.[2] To the present writer none of this seems sufficiently fundamental. What is of value in the case for manufacturers' advertisement is that it supplies information, and that it sometimes concentrates demand on a few styles. But both these services could be supplied equally by retailers, at less cost, and more impartially, especially if greater steps, voluntary or compulsory, were taken to promote standardisation, and more so still if something like Chamberlin's advocacy of trade mark infringement proved practicable.[3] It should not be on the manufacturer's strident claims that the public has to rely for information. The retailer, usually a better judge of merchandise than the consumer, is better able to decide which brand is worth buying in large quantities, and pushing. In the view of the present writer, while branding should be retained to facilitate identification, manufacturers should be allowed to advertise only in trade journals for circulation amongst wholesalers and retailers, and not in the ordinary press. In this country we have already banned advertisement from the wireless, will, it is to be hoped, ban it from the sky, and are always talking about banning it from the hoardings. To ban it from the ordinary press would not be as revolutionary as it sounds, and would also greatly increase the perfection of the market, and reduce waste.

There remains, however, a further check on retailers which prevents them from carrying as small a range of styles as they might otherwise desire. It is the desire of their customers to be able to choose from as wide a range as possible, and indeed, in some cases, notably women's wear, the desire to have something different from what other people will have. In some cases, in a large shopping centre, this does not matter. If there are enough people who prefer cheapness and standardisation to variety, some retailers will be able to concentrate on mass lines, and to secure for such consumers all the economies of scale. But in other trades it may remain the case that if all customers were compelled to accept the standard brand, costs would be lower

[1] For a discussion of American attitudes and practices see Reid, M. G.: *Consumers and the Market*, Part VI.

[2] Cf. the discussion by Plant, A.: "The Distribution of Proprietary Articles," in *Some Modern Business Problems*, ed. Plant.

[3] Op. cit., Appendix E.

than they could be without such compulsion. Should such compulsion be exercised ? This is clearly a question for the philosopher rather than for the economist.

5. We come, thirdly to delivery costs. That the several vans of different shops should be delivering in the same road has long seemed a conspicuous source of waste. The waste, however, such as it is, consists not in the fact of delivery, but in the inefficiency with which delivery is organised. If shopkeepers would use, not their own delivery vans but the services offered by public transport contractors, costs would be reduced, as each contractor could deliver on behalf of several different shops. Competition between the shops would not be affected by this any more than competition between manufacturers is affected by common use of the railway system or the post office. That they do not use public transport facilities is due principally to the fact that the convenience of delivering for oneself (plus the slight advertisement value of having one's own vans) outweighs what would be saved. This applies equally to pooled delivery services, such as have been enforced in wartime. Moreover, there is reason to believe that the extent of possible savings is grossly exaggerated. The case most quoted in England is milk delivery. Yet an independent investigator, who made a careful survey in Battersea, concluded[1] :—

> " To limit the number of distributors to a maximum of four in any road would cause such a small increase in the sales of these four as to have an almost negligible effect upon costs. To limit the number to two in this area would increase the sales of the two largest distributors by 66 per cent., but one is not convinced that even to limit it to one distributor would materially reduce the large distributor's costs. With the largest distributor only a small portion of the roundsman's time is taken in covering the actual length of road ; most is taken in going from door to door, in bringing the customer to the door and engaging her in conversation. In one respect only does it seem probable that giving a particular area completely to one milkman would materially reduce costs, and that is in preventing people from buying from two or more distributors. In this area 11 per cent. of the families bought from two or more distributors ; in Oxford Dr. Murray found nearly 15 per cent. dealt with two dairymen. This seems a complete waste, and probably its abolition would reduce distributive costs by 10 per cent., say at least 1d. per gallon.

The principal cause of waste in milk distribution, of course, is the fact that the authorities have maintained the minimum retail price at a level allowing a margin so grossly in excess of the costs of large and efficient distributors that an altogether excessive number of firms is able to exist, and also an excessive amount of service.[2]

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[1] Booker, H. S. : " A Survey of Milk Distribution ", ECONOMICA, February, 1939.

[2] The facts are given in two official papers : *Report by the Food Council to the President of the Board of Trade on Costs and Profits of Retail Milk Distribution in Great Britain*, 1937, and *Report of a Committee appointed to examine Costs of Milk Distribution*, 1940.

6. Finally we must consider the proposition that competition causes service to deteriorate. The shopkeeper, it is said, pressed by competition, and unable to make both ends meet, is driven to reduce the quality of the product and the service, and to misrepresentation and fraud.

To say that the quality will deteriorate is only another way of saying that the price will rise in competition, and we have already examined the conditions in which this is possible. In a really competitive market a shopkeeper may offer less value for the same money, but this will usually be on his way out. If consumers are unable at the time of making a purchase to distinguish between good and bad quality, a retailer may pass off bad as good, but the consumer will discover the difference on using the article, and retailers who do this will lose goodwill and disappear. It remains possible that there are articles whose quality the consumer cannot distinguish even in the course of use, and which he buys relying solely on the retailer's advice, or on high price as a guarantee of quality. Much is made of this by some writers, quoting quality tests which show low correlation between quality and price. The range of articles to which this applies cannot be very great, and would be smaller still if some teaching on how to use the market were supplied to the consumer through the press, the wireless, films, women's institutes, guilds, unions, and other media for adult education.

That competition may stimulate fraud cannot be questioned. In some countries the primitive view prevails that every act of sale is a battle of wits, and even the strictest laws prohibiting adulteration or controlling weights and measures or the like cannot entirely outlaw sharp practice. Honesty is the best policy only where shopkeepers are competing in honesty ; not where they are competing in dishonesty. Fortunately in this country the competitive forces work in the right direction. Especially is this so where the shopkeeper depends on the regular support of a loyal clientele, and cannot afford to lose their goodwill. It is less so where the customer will buy from any seller infrequently—perhaps only once—as in buying durable goods or buying in the course of a journey. Here the shopkeeper may be tempted to follow too literally the biblical injunction to take in the stranger. If he does this consistently he will probably sooner or later get a bad name and disappear ; but he may get away with it, if done infrequently, or may be replaced merely by someone else who follows the same policy. The best safeguard for the public is to learn to shop around making enquiries before committing oneself to the purchase of an expensive durable good.

7. We may now summarise the conclusions of this section as follows :—

(*a*) In large shopping centres it is unlikely that more service is provided than is wanted ; competition between different forms of retailing tends to prevent such waste.

(*b*) In smaller suburban centres some customers may receive some service which they would rather not have; others, through price discrimination, receive more service than they are willing to pay for, but the effect of this is to make shops nearer ideal size.

(*c*) All forms of retailing might have lower costs but for manufacturers' advertisements to the public, and but for the public's desire for variety of styles. How far these should be controlled, however, raises issues independent of the degree of competition in retailing.

(*d*) Service may deteriorate as a result of competition, but this is not likely in this country.

III. Unfair Competition

1. In this section we are concerned with arguments that certain forms of competition are unfair and distort the distributive process. The practices in question are (*a*) claiming the status of a retailer without fulfilling all the functions; (*b*) "poaching" in one trade by retailers in another (e.g., grocers selling patent medicines); (*c*) direct sales to the public at "wholesale" prices; (*d*) "special" prices to large traders; (*e*) gift coupons and trading stamps; and (*f*) sales to co-operative societies.

2. We can begin with two points common to all these problems. The first is that the retailer's criterion of what is fair is not the economist's. The retailer characterises as unfair almost anything which upsets his expectations, especially if it does so by diverting trade to a different form of retailing. The economist cannot condemn a practice solely on these grounds.

Secondly, underlying most of these cases is the belief that distortion of the "normal" channels of distribution raises costs. Coupon trading is a good example of this. If chocolate manufacturers give away cameras in redemption of coupons, the retailers of cameras claim that this reduces the sales of cameras through existing outlets, and therefore increases costs. All the forms of "unfair competition" listed above would be attacked on this ground, among others.

For this line of argument to be valid, four propositions must be established. First, that the new form of retailing does diminish the business done by the old. Secondly, that when the business done by the old diminishes, the effect is an increase in its costs, i.e., that the number of shops does not diminish *pari passu*, the remaining shops having the same average turnover as before. Thirdly, that the costs of the new form exceed the marginal costs of the old. And fourthly, that the difference in costs is not justified to the consumer by difference in service or other convenience.

It is not always accepted that the new form of retailing diminishes the business done by the old. For example, some supporters of gift coupons claim that the persons who get cameras through this medium would for the most part not be willing to buy cameras, and that having got the cameras they buy films from photographic dealers and have them developed, so that the trade of the ordinary dealer is not reduced but increased. It is always possible in some cases that the extension of a new cheap form of retailing may so expand demand that even the older and more expensive forms of retailing also benefit (just as publication in the " Penguin " series sometimes increases sales of the expensive edition of a book), but there must also be many cases in which the net effect is that the turnover of the older forms is reduced.

If this is the case, the effect will be different in large shopping centres and in small. The large shopping centre being defined, for any given commodity, as one where the number of customers is large enough to support several shops each of optimum size, the effect will be simply that one or more of these shops is put out of business, the others keeping the same size, costs and prices. In the smaller centres, however, the effect is different. Unless the new form replaces the old form altogether, its effect is to detach some customers lying within the area of each shop. This is a reduction in the density of the buying population, which, as we have already seen, results in smaller shops and higher costs.

When this is the case, some customers are benefiting from the new form of competition at the expense of others who pay higher prices. If it were possible for all customers to transfer to the new form, the public would not be harmed by this. For example, if it could be shown that the effect of the spread of co-operatives was not simply to bankrupt some private traders, but rather to raise the prices paid by non-co-operators, no economist would justify restrictions on co-operatives on this ground, since it is open to all customers to join co-operatives. But the case is different with coupon trading ; we cannot advise all persons who want cameras to get them through eating chocolates. Where the cheaper form of distribution is not open to all, the problem is more difficult.

The key to the solution is total costs. If the sum of the costs of the total sales under new and old systems is less than it would cost to distribute the same output through the old system exclusively, the new form is justified. This will be the case so long as the costs (prime plus fixed) of the new form of retailing are less than the marginal costs of the old. But the new form will not be justified even if its average cost (and price) is less than the average cost (and price) of the old if it is not also less than the marginal cost of the old. In these conditions a case can be made for prohibiting the new form of retailing. The problem arises only because some retailers who have falling average costs cannot discriminate in favour of the type of customer to whom the new form of retailing makes its restricted appeal. If they could discriminate they could prevent the new form from establishing itself by quoting prices, based on marginal cost, less than the new form could support. As they cannot discriminate, the new form emerges, and social costs are increased.

To complete the argument it must be shown that the customers who would patronise the new form would gain only to the extent of the lower price; that there is no special convenience or service attached to the new form, such as would justify higher social costs; in a word, that the two forms of retailing are nearly perfect substitutes. Thus the suburban shopkeeper cannot argue that the cheap shops in the centre should be closed even if he could show that their average costs exceed his marginal costs, for shopping in the suburbs is not an exact substitute for shopping in the centre, and many people who live in the suburbs would find it more convenient to shop in the centre even if prices were the same. Nor, to turn the argument round, can the cheaper forms of retailing apply a similar argument for eliminating the more expensive; the two are not exact substitutes. For the argument to be applicable it must be shown that the price is the only practical difference from the consumer's point of view.

It seems a revolutionary conclusion that costs can sometimes be reduced by eliminating the "cheaper" forms of retailing, but as we shall see, the practical significance of the conclusion is very limited. Let us now take each form of "unfair" competition in turn.

3. A number of retail trade associations in this country have agreed with manufacturers or wholesalers that a person should not receive retailer's discounts on his purchases unless he has or is prepared to have a regular shop, open at the usual hours, carrying certain minimum stocks, and perhaps offering after-sales or other usual services. This "fair trading" policy, as it is called, is found, for example, in the bookselling, photographic and motor car trades.[1] This sort of policy is aimed partly at the small part-time retailer, who keeps a parlour shop, partly at "poaching" in one trade by members of another, and partly at direct sales to the public at wholesale prices. It thus covers the first three cases on our list.

It is most unlikely that this restriction could be justified on the ground that the average costs of those excluded exceed the marginal costs of those that remain. The real costs of the man with the parlour shop are low, because his transfer costs and rent are low. So also are the costs of the "poacher", who enters the extra trade usually because he has already under-utilised premises and staff for his own trade. From the point of view of real costs, one can say broadly that the more "poaching" there is the better, since this gives the average shop in each district a larger total trade and lower costs (the limit to "poaching" is set by the advantages of specialisation). And as for sales direct to the public at wholesale prices, this is the case least likely to fit the argument, since the consumer's cost of retailing to himself is zero (though he may have a heavier transport cost). The

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[1] The fullest survey of the facts about each of the principal retail trades is to be found in a series of articles published in the *Manchester Guardian Commercial Weekly* in the second half of 1938 and the first half of 1939. For books and motor cars the relevant dates are 24/6/38 and 28/10/38.

" fair trading " policy cannot therefore be justified on the ground that sales through these channels are uneconomic.

Neither is the justification usually attempted on these lines. First, in some trades the retailer says, in effect, " To do my business properly I must carry a wide range of models, to offer the public a good choice ; it is unfair that I should lose trade to someone who does not do the same ". It is not unfair that some shops should stock less wide ranges than others ; those customers who wish to be able to consult a wide range are free to do so at the more expensive shops, while others may chose the cheaper. But the argument is more subtle. The retailer with the wide range argues that other retailers benefit from his policy ; sales in the other shops are larger because there are shops displaying a wide range ; in effect the retailer with the wide range is bearing part of the " information " costs of the whole trade. The argument is unimpressive ; no retailer is compelled to act in this philanthropic way. If he really thinks that it is possible to thrive off other people's stock carrying, there is nothing to stop him from doing so. This, however, brings us to the second argument. Manufacturers frequently support the fair trading policy because they think that it is in the interest of the trade as a whole that retailers should carry a wide range of stocks ; it has advertising value, and causes total sales to be greater. In discriminating against retailers who do not carry this range they are in effect paying a subsidy to others for advertising. The desirability of this rests therefore on the desirability of advertising, and in particular of advertising simply to divert demand from other commodities to the advertised. This can hardly be justified as " information ". The public knows of the existence of the particular class of merchandise, and multiplication of the varieties within the class in order specially to catch its eye deserves no special protection. The third argument is more weighty. It is that in certain trades the retailer should have special qualifications, e.g., that in the photographic trade the public would be badly served by retailers who gave bad advice on the respective merits of cameras, or bad after-sales service. Where the commodity is purchased frequently, the public needs no such certificate of qualification ; the buyer soon finds out for himself whether his grocer is to be trusted. But where the service is infrequent, some certificate would be useful ; e.g., how is the householder to know whether the radio dealer round the corner can really mend a silent wireless set without at the same time doing irremediable and temporarily concealed damage inside ? There is something to be said for requiring retailers in certain durable goods trades to pass some test of competence, particularly in the photographic trade, radios, and garages, as is already done for chemists ; or at least for an optional test, enabling the public to distinguish between certified and uncertified. But it is a fundamental principle that such tests must be conducted independently, and not by interested parties, such as manufacturers or a retail trade association.

Lastly, sales to the public at " wholesale " prices are opposed on the ground that it is unfair that the retailer should face such competition from the people who sell to him. This is indeed a common complaint, especially in the drapery, sports goods, cycle, meat, jewellery and outfitting trades,[1] and includes protests against sales to members of clubs at special prices. The saying that " only saps pay retail prices " indicates how common is the practice. A manufacturer (or wholesaler) may adopt it in one of two circumstances. It may be that he is able to distribute to the consumer at a cost less than the margin the retailer claims. If the retailer's margin barely covers the retailer's costs, this means that the manufacturer is able to distribute more cheaply by direct methods of sale, and there is no case for preventing him from doing this. But it may be that the margin exceeds retailers' costs, and that the efficient retailers are prevented from reducing their prices by a resale price maintenance scheme. It would certainly be unfair of manufacturers to undersell retailers who are compelled to maintain prices, but the problem this raises is whether prices should be maintained in this way, and this is reserved for the final part of this paper.

Alternatively, the manufacturer may be discriminating, and earning a higher profit on his sales to retailers than on his sales to the public. This is unlikely to be profitable if it results in sales to the public expanding at the expense of sales to retailers—and if it does not the retailer has no ground for complaint, because the manufacturer is then substituting less for more profitable sales, unless the expansion of direct sales is much greater than the decline of sales through retailers.

A similar problem arises in connection with the allegation that large retailers flourish as a result of obtaining special prices. This problem has arisen much more acutely in the United States than it has in this country, where the multiple shops are much less aggressively competitive than their American counterparts. The rôle of the chain in the American distributive set-up is played in this country in a different way by the co-operative movement. Multiple, chain and co-operative owe a great part of their success to buying more cheaply than the independent shop. That there are real economies reflected in the lower prices they pay is not open to question ; the large retailing organisations relieve the manufacturer of many costs he would incur if selling to small shops, and they perform the wholesaling functions for themselves more cheaply and efficiently than the wholesale firms do for the small independent shop. Nevertheless it is possible that they also get discriminatory prices, based on their great bargaining power. In the United States, where the problem has been widely debated and officially investigated, the evidence is not conclusive.[2] Three reactions may be noted. The first is the " classification " of customers in trade categories. Here, trade associations bring pressure to bear on manufacturers to base their discounts on the trade status of the buyer rather than on quantity bought, so that the price charged for any given quantity will be different according to whether the buyer is a consumer, a retailer, or a wholesaler. This policy is clearly uneconomic from the public point of view, since it hampers the development of efficient large scale retailing, and tends to ossify the structure of distribution and to preserve intermediaries beyond the extent justified by the services they perform. The second possible reaction is legislation against price discrimination ; in the United States the Robinson-Patman Act is intended specially to protect the small shopkeeper against discrimination. The object of such legislation is laudable ; some discrimination between consumers can be defended, where average costs are falling, if it brings output nearer the ideal (especially if the discrimination is in favour of the lower income groups), but discrimination between retailers and consumers or between different classes of intermediaries is unfair in itself, as well as likely to cause an uneconomic distortion of the distributive structure. To prevent it by legislation is nevertheless a formidable task. Rather more perhaps may be hoped from the third reaction. This is for retailers to co-operate in buying, so as to secure the full economies of scale. The co-operative retail buying movement has spread very rapidly in the United States, where it has saved the small shopkeeper, but has made little progress in the United Kingdom, where small retailers prefer the protection of " classification " and price maintenance. Co-operative buying is undoubtedly the most desirable reaction from the public point of view.

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[1] See the relevant articles in the *Manchester Guardian Commercial Weekly*, of 12/8/38, 16/9/38, 23/9/38, 11/11/38, 18/11/38 and 2/12/38. For an American discussion see Tallman, G. B. : " When Customers buy at Wholesale ", *Harvard Business Review*, XVII, p. 339.

[2] For a summary of the discussion see Twentieth Century Fund, op. cit, p. 83.

4. Retailers object to the distribution of goods through gift coupons and trading stamp schemes because it interferes with their trade. No case can be made out on the basis of real costs. The real costs of handling the goods distributed in this way seem to be rather low, as the work is done on a large scale ; on the whole, the customer gets good value for his money. More interesting is the argument that the use of coupons and trading stamps increases market imperfection ; customers are tied to particular commodities by coupons, or to particular retailers by trading stamps, and may have their attention diverted from the quality of the goods they are buying, or the prices charged, by the illusion of " getting something for nothing ". Some countries have indeed prohibited or restricted coupon trading, usually in order to protect retailers, but the Board of Trade Committee which investigated the practice in 1933[1] concluded that the public suffers no harm, and no new evidence or arguments have since emerged to overthrow this conclusion. Coupon trading, however, is just an adjunct to non-price competition between manufacturers and to manufacturers' advertisements ; and the trading stamp is primarily the product of resale price maintenance. If these sources of market

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[1] *Report of the Committee on Gift Coupons and Trading Stamps*, Cmd. 4385 of 1933.

imperfection were removed most of this form of trading would disappear.

5. The view that the consumers' co-operative movement is in some sense unfair still seems to persist in some quarters in this country, though recent events suggest that it does not turn many votes! To discuss the argument that the principle of co-operative trading is itself unfair would strain the reader's patience. Only special points deserve mention. One is the suggestion that the co-operative surplus is to some extent due to special prices secured from manufacturers by bargaining power. There is no evidence of this. The exhaustive enquiry published in 1938[1] showed that the surplus arises out of the real economies of co-operative trading, especially lower publicity, rent and interest costs, but above all wholesaling economies. A second point is the insistence of some retail trade associations that where resale prices are maintained it would be unfair to allow co-operatives to pay dividends on purchases of these goods, since the dividend would represent a form of undercutting for which private traders would be penalised. There is some substance in the argument, but its final validity depends on the case for price maintenance, which we shall discuss in the last part of this paper. Finally, it is argued that it is unfair that co-operative dividends are not subject to taxation. The literature on the point is large. The opinion of the present writer is that the co-operative surplus is not comparable with the private trader's profit. That profit contains two elements, remuneration for his labour, and interest on his capital. The comparable items in co-operative trading are the salary paid to managers and the interest paid on shares; the surplus partakes of neither of these elements, but is rather a form of voluntary saving. This view is not, however, accepted universally by economists.[2]

6. We may therefore conclude as to unfair competition that the only valid and significant case is that against price discrimination between classes of buyers, in accordance with trade status; price differentiation should be based rather on the cost of handling quantities.

IV. Limitations on Competition

1. To many traders what is unfair is not just certain forms of competition, but the very fact of competition itself, whether it be competition in price, service competition, or the entry of new firms into a trade. Several retail trade associations operate schemes to enforce resale price maintenance, and many would like to imitate the newsagents in enforcing a distance limit.

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[1] Carr-Saunders, A. M., Florence, P. Sargant, and Peers, R.: *Consumers' Co-operation in Great Britain*, esp. Chs. XXII and XXIII.

[2] For discussion see Macgreggor, D. H.: "The Taxation of Co-operative Dividend", *Economic Journal*, March, 1933, and Fay, C. R.: "Co-operators and the State", *ibid.*, September, 1933.

2. It is not surprising that most retailers of convenience goods, operating as they must on a small scale in local markets, favour price maintenance. It seems to offer them protection from the large shops, of optimum size, in the large centres, who, correspondingly, are on the whole against price maintenance. The protection is illusory. In price maintenance the margin is set normally high enough to cover the costs of most retailers in the trade, and, if competition does not whittle profits away in more costly service, either more retailers are attracted, or " poaching " spreads ; the number of outlets increases, and turnover per outlet falls ; costs per unit of sales rise ; pressure is applied for a still higher margin ; this attracts still more outlets ; and the cycle continues. From the point of view of the retailer, even the highest margin will barely cover his costs, because the number of outlets, and turnover per outlet, will be adjusted to whatever the margin may be. So long, therefore, as there is free entry into a trade, directly or by " poaching ", price maintenance is no protection from competition ; it is merely a costly and wasteful burden on the public. The drug trade is the stock example of what happens. Before price maintenance was adopted, the number of outlets for patent medicines more or less equalled the number of chemists ; to-day, because of the attractive margins, other shops have taken up patent medicines, and outnumber the chemists in this trade by ten to one. Similarly in the tobacco trade there are 23 " poachers " to every one tobacconist, and in the confectionery trade five " poachers " to every " legitimate " confectioner. The chemists have now been driven to bringing organised pressure to bear on manufacturers to restrict the sale of patent medicines to qualified chemists.

Surprise has been expressed that manufacturers should support price maintenance, instead of wishing their goods to be retailed as cheaply as possible. Fear of the repercussions of the " loss leader " has some significance, but only in relation to the more fundamental explanation. The fact is that in many classes of trade sales depend more on there being many outlets than on low prices. This is the reason why the state has for centuries restricted the number of public houses, and it applies with equal force to confectionery, newspapers, books or cigarettes. Moreover, within any of these classes of trade, it may be of greater advantage to a manufacturer to have his brand available in every shop, than to have it available at a low price. These advertising considerations make manufacturers willing to fix margins high enough to encourage even the least efficient and smallest retailers to stock the brand, and to take the elaborate steps necessary to avoid a differentially low price level or loss leaders in the large central markets which would draw trade from the smaller shops and discourage them from stocking the brand. For shopping goods, which are sold mostly in large shopping centres where sales depend on keen prices rather than on wide stocking, resale prices are never likely to be fixed.

From the public's point of view resale price maintenance is doubly condemned (*a*) because it causes the number of shops and the amount of service to be excessive and prices to be too high; and (*b*) because manufacturers promote it for advertising reasons. And the fact that it does not really in the long run shelter the shopkeeper from competition simply removes any ground for sympathy, and makes the system all the more absurd. It is hardly necessary to consider two arguments which are sometimes advanced; that the public gains from price maintenance because the system tends to enforce a maximum price as well as a minimum, so that the customer cannot be defrauded; or that the public would not have " confidence " in a commodity found selling in different shops at different prices. If it is maximum prices that benefit the public, then fix not minimum but maximum prices; and what happens to public confidence in that wide range of commodities whose retail prices are not fixed?

The case against price maintenance is beyond doubt. It is one of the major sources of waste in distribution, and the public would benefit greatly if it and the boycotts, stop lists, discrimination against co-operative societies, and other paraphernalia by which the system is enforced were made illegal.

3. The licensing of shops, however, raises more difficult problems. We have seen that, even if prices are not maintained, the number of suburban shops may be excessive. A *prima facie* case for restriction is therefore established.

Any such proposal, however, has to meet a formidable array of objections. First, many people are anxious to maintain an open door in retailing as a refuge for the small man from " wage slavery " into " independence ". In many quarters the small trader is regarded as the pillar of society, a view which cannot always survive closer examination of his outlook and prejudices. But, whatever the merits of the small trader, it is certainly not kind to encourage large numbers of small men to enter into retailing. By and large, the costs of small shopkeeping are high, and the rewards are correspondingly low, frequently much less than wage employment would yield. Neither is it a profession for the unskilled; the ignorance of small retailers is one of the principal reasons for their high rate of bankruptcy. In the United States it has been estimated that half the retail shops opened in any year fail within twelve months, and that the average life of a shop is only five years.[1] Similar information is not available for this country; thanks to price maintenance and a less aggressive atmosphere of competition the mortality rate is probably lower, but it is without doubt very high; to go into retailing is one of the easiest ways of losing the savings of a lifetime. Those who wish to keep retailing open specially to attract the small man seem therefore to be even more dangerous to the small man than they may be to the public.

It is more important to consider whether restricting the number of shops really would benefit the public. At present there is some competition in prices and in service. Restriction of entry might lead merely to an increase in shopkeepers' profits and prices, and to a decline in the quality of the service. The war has taught us how quickly the quality of the service deteriorates in retailing when once the spur of competition is removed, and many members of the public would think that some excess in the number of shops is a small price to pay in order to secure the benefits of competition.

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[1] Temporary National Economic Committee, Monograph No. 17, *Problems of Small Business* esp. Ch. II.

The structure of the distributive trades would also be gravely affected by restriction of entry. For the past century the efficiency of retailing has been improving largely through the expansion of large scale retailing—co-ops, multiples and department stores—relatively to small scale retailing. Large scale retailing is able to operate on margins of anything between 30 per cent. and 50 per cent. less than those required by small retailers. The small retailer has, indeed, his place in the industry, especially in the sale of shopping goods, where standardisation is not important, and in the sale of convenience goods in small shopping centres, and especially in building up a clientele on the basis of a high quality of personal service. This place would be even larger than it is if the efficiency of small shopkeepers could be improved, e.g., by better training facilities. A woeful ignorance of business principles is one of the chief causes of the wastefulness of small shopkeeping and the high mortality rate. For example, most small shopkeepers try to carry too wide a range of styles, brands, etc., deal with too many wholesalers and manufacturers, and offer too much service, for the limited turnover which they can secure. And the small man would be still more certain of his place if he would turn his eyes from the illusory safety of price maintenance and "classification" to the possibilities of standardisation and co-operative buying, thus challenging the large scale retailer on his own ground. There is undoubtedly room for small scale retailing. But what case is there for measures which extend it an artificial protection by preventing the expansion of large scale retailing, which has been the spearhead of improved efficiency in distribution ? The writer is as sensitive as any, and more sensitive than most, to the values of a society in which economic power is widely dispersed through the predominance of small business. But he is highly sensitive, too, to the persistence of poverty, and the need to increase wealth by utilising as far as possible the more productive types of enterprise. To bolster up small scale retailing beyond its proper place in the distributive structure is to condemn an important part of our economy to waste and inefficiency. It should not be forgotten that one person in seven is engaged in the distributive trades.

More probably, this issue would not arise. For restriction of entry would give the multiples an incentive to buy up more and more small shops—small shopkeeping might not be preserved, but on the contrary might disappear. Nor would the public benefit ; the small retailer, protected by his licence, would refuse to sell except for a handsome price, which the multiple organisation would be willing to pay, relying on the licence, and the net effect would be to raise prices against the public. Not only the multiples would play this game, but also the manufacturer. The public house is a standing example of the incentive the licensing of retail outlets gives to manufacturers to integrate forward and buy up the outlets for monopolistic purposes. Probably the principal result of restricting entry into retailing would be that multiples and manufacturers would buy up the shops, that monopoly would be greatly increased in manufacturing, and that the public would lose more in higher prices and reduced service than it loses from freedom of entry. Though there are too many shops if there is freedom of entry, merely to restrict freedom of entry would probably make the public poorer than before.

4. What causes waste in retailing is not competition but the fact that price competition is not sufficiently effective, especially in the convenience goods and suburban trades. This is due in the first instance to resale price maintenance, the abolition of which would enable the more efficient retailers to expand and to reduce prices to the consumer, and would reduce the number of shops, releasing resources for other urgent purposes.

But the number of shops might still be too large, and the average shop too small, even if price maintenance were abolished. Figure I shows the position. In the absence of price competition the average shop will have a size well to the left of *N* (in terms of Figure III it will be at *K*) ; price competition will increase the size to *N* ; but the ideal size is given by *M*. Elasticity of demand will bring the competitive size nearer *M*, but not necessarily right up to it. Theoretically, the simple way to achieve the right number and size of shops is to fix a maximum gross margin *QM*, whereupon the number of shops will contract, and average size increase to *M*. But there is nothing simple about this solution in practice. There is in reality no average shop ; different gross margins are appropriate in different conditions of population density and different degrees of service. The same margin cannot be fixed for every retailer. Neither is there an average commodity ; even within a single class of merchandise different articles need different margins, according to the time it takes to handle them, their size, rate of turnover, and so on. It is true that margins have been elaborately controlled during the war, but no one can pretend that the hit or miss methods used in war-time would be very suitable for permanent peacetime regulation.

It may be suggested that the simplest way out is to fix a maximum margin only for one standard and popular type within each class of merchandise, in the expectation that the margins on all other types will have to correspond. This might work so long as retailers could not escape selling the selected brand. If " utility " production is continued, this can be tried, the government allowing such low margins that only the appropriate number of shops remains in business. The retailers may be expected to fight the attempt by starting a whispering campaign against the " utility " articles, and persuading consumers to buy only uncontrolled articles. The government would win so long as it retained power to withhold labour and raw materials from non-" utility " production, but as soon as non-" utility " lines could be freely produced, the limited price control would lose much of its effectiveness.

There is one other theoretical alternative to price fixing, which is equally impracticable, i.e., to give the co-operative societies a monopoly of retailing, or at least a monopoly of retailing convenience goods, since it is in these rather than shopping goods that there is excess capacity. If they had a monopoly of retailing the societies would have no incentive to establish more or less than the right number of shops. The proposal is specially attractive because, whatever their shortcomings in handling shopping goods, the co-operative societies are unquestionably the cheapest distributors of convenience goods in this country, and they would be cheaper still if certain managerial and structural reforms could be effected.[1] But it is politically quite impracticable to establish any such monopoly.

5. There is in fact no practicable solution to the problem of " excess capacity " in retailing, in the sense of reducing the gap between the competitive number and the ideal number of shops. This, however, need not daunt us, for two reasons. First because " excess capacity " exists only where elasticity of demand is insufficient to bridge the gap, on which we have no information. And secondly because, in either case, the actual number is so far in excess of the competitive number that if we could get it down to the competitive level we should have made notable progress. The way to this level is simply to make price maintenance illegal. This is the most urgent reform needed in retailing ; its repercussions would revolutionise the distributive structure.

Democrats may rejoice that one result would be a rapid expansion of the consumers' co-operative movement in many trades for which it is specially suited. Here is a movement to which more than half the families in the country belong. Freed from the trammels of price maintenance, and inspired with a sense of its mission, this movement could set prices all along the line at levels which would drive waste out of distribution. It is the pride of the movement that it protected the consumer in the days when adulteration, short weight and fraud were the bugbear of retailing. To protect him from waste would add another page to an already noble history.

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[1] For a discussion of needed reforms see Carr-Saunders and others, op. cit., especially Part IV.

Recent Contributions to General Equilibrium Economics

By J. R. Hicks

1. Confronted with the task of reviewing these two very interesting books,[1] I find myself in a peculiar position. They are both of them concerned with the same line of country as I explored in my *Value and Capital*, and they use, on the whole, closely similar methods. I naturally find myself most interested in those parts of their work which come closest to mine, and particularly in those parts where their conclusions differ from mine. It is obviously these parts on which the authors themselves would most like me to comment, and probably the same thing will be true for their readers. This is accordingly what I propose to do. It means giving their books a somewhat one-sided, and I fear very egotistical review; but it seems to be the right thing to do in the circumstances.

Dr. Mosak's book purports to be concerned with the application of General Equilibrium economics to the theory of international trade; but in fact less than one-third of his book is concerned with that application. And, speaking as impartially as I can, I do not think it is the most interesting third. There does not seem to be much to be done about the application to international trade except a fairly mechanical reinterpretation of the general theory in terms of this special case. All the heavy guns do little more than confirm conclusions already established (more simply and more directly) by Professor Robertson. The part of Dr. Mosak's book which does seem to me to be important is that in which he develops the general equilibrium theory as such, and introduces into it some distinct novelties.

There are two main ways in which Dr. Mosak's theory differs from that presented in *Value and Capital.* One of them relates to the static theory of the working of general exchange equilibrium. The other is concerned with the alleged "imperfect stability" of the dynamic system. Professor Lange has actually built the whole of his book round the second of these two topics. I have therefore two matters on which to comment in respect of Dr. Mosak, one only in the case of Professor Lange. But the second issue, with which both my authors are concerned, is much the more important and the more difficult. I shall therefore begin by getting the first of the two out of the way, and then I shall devote the rest of my remarks to the larger question.

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[1] *Price Flexibility and Employment*, by Oscar Lange. Principia Press. 114 pages. $2. *General Equilibrium Theory in International Trade*, by Jacob L. Mosak. Principia Press. 187 pages. $2.50.

2. Some time after *Value and Capital* was published, I became aware that there was a slip in my analysis of static exchange equilibrium. I had failed to make use of all the conditions at my disposal, and I was therefore left with a number of *queer* cases which ought to have been ruled out. I drew attention to this slip in a footnote to an article[1] published in 1942. I intend to make the correction in any future edition of the book, and have in fact requested that it should be made in the Spanish translation which has recently been published.[2] Perhaps I may be allowed to quote from the preface which I wrote for this translation last year :—

" I have taken advantage of the appearance of this translation to introduce a few improvements which have not yet been incorporated into an English edition. The majority of these are concerned with a particular mathematical inference which I had overlooked, and which only came to my notice when I was working on a related subject some time later. In the original edition I maintained that instability of exchange equilibrium might arise from two causes: asymmetrical income effects and ' extreme complementarity '. The first of these causes undoubtedly makes sense ; it is not difficult to construct particular instances showing how it would work. But it was difficult to make sense of the second, though I felt bound to retain it in my argument, as it seemed to be implied by my mathematics. I have now discovered, however, that it is not implied by the mathematics ; there is a mathematical reason, which I have overlooked, why it is ruled out. I have therefore suppressed this second ' cause ' altogether, so far as the text is concerned—to my own considerable relief, and (I confidently expect) to the great convenience of the reader. The reasons why it can be suppressed have been inserted into the mathematical appendix.

" Another little ' knot ' in the original version—where a ' literary ' argument in the text (pp. 221–2) seemed to lead to a different conclusion from that given in the mathematical appendix—disappears for the same reason."

Dr. Mosak has got all this straightened out. Having done so, he has gone on to provide a mathematical proof of a principle which I established more loosely—that in an economy where income effects cancel out, and where no complementarity is present, a rise in the excess demand for X in terms of the numéraire will cause the prices of all other commodities to rise in terms of the numéraire, but proportionately less than the rise in the price of X. In connection with this proposition, he introduces a suggestive change in terminology. According to the basic theory (common to us both) the effect of a change in the price of one commodity on the excess demand for another contains two terms, an income term and a substitution term. The definition of substitutes and complements turns on the sign of the substitution term. Dr. Mosak, however, speaks of the commodities being " gross substitutes " or " gross complements " when he is concerned with the sign of the two terms taken together, that is to say, with the sign of the total effect. (It will be noticed that *gross* substitution and complementarity are not symmetrical ; the fact that X is a gross substitute for Y does not imply that Y is a gross substitute for X.) He then goes on to show that the effect of a change in the *excess demand* for one commodity on the *price* of another depends upon a term which is the co-factor of the gross substitution term in a certain Jacobian (actually the general price-quantity Jacobian of the whole system). He therefore speaks of Y being an " inverse gross substitute " for X if an increase in the excess demand for X induces a *rise* in the price of Y ; while if there is a fall in the price of Y, Y is an " inverse gross complement ". The fundamental proposition that a rise in the excess demand for X in terms of the numéraire will raise the prices of all other goods in terms of the numéraire if no complementarity is present, can therefore be expressed in a slightly more general form by saying that if all commodities in the system are gross substitutes, they will also be inverse gross substitutes ; but if any pairs are gross complements, then it does not necessarily follow that any other pair which are gross substitutes are also inverse gross substitutes.[1]

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[1] " Consumers' Surplus and Index Numbers," *Review of Economic Studies*, Vol. IX, p. 133 note.

[2] Valor y Capital ; version española de Javier Mârquez, Mexico City (Fondo de Cultura Economica) 1945.

This is good terminologising ; for it expresses in terms of substitutes and complements the fundamental " Principle of Duality " which underlies all general equilibrium economics. The theory of general equilibrium under perfect competition can be worked out, as I worked it out in *Value and Capital*, using prices as parameters ; or alternatively it can be worked out, as Hotelling did in " Edgeworth's Taxation Paradox,"[2] using quantities as parameters. The concepts which arise in the two formulations are not identical, though they may be identical in some simple cases.[3] Yet there is always a definite relation between them, akin to the geometrical relation of reciprocity. " Literary economists " must beware of the danger of mistaking one for the other.[4]

It is probable that of the two formulations, that which proceeds by quantity-parameters will ultimately prove to have the greater range. It is more extensible to cases other than those of perfect competition, and it is probably of more use for the problems of welfare economics. Nevertheless, we shall never be able to use these instruments easily until we have a clear idea of the two routes and of their inter-relations. Towards that clarification Mosak has given us considerable help.[1]

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[1] Mosak, pp. 45, 49–51.

[2] *J. P. E.*, 1932. Incidentally Mosak has some useful pages (47–9) on the Edgeworth-Hotelling paradox itself.

[3] An interesting illustration of this duplication, in a field quite different from that examined by Mosak, is the distinction between " price-variations " and " quantity-variations " in theory of Consumers' Surplus. (See my " Four Consumers' Surpluses ", *Review of Economic Studies*, Vol. XI.) This is another case in which the distinction between the pair of magnitudes is easily overlooked, and was in fact completely overlooked until recently.

[4] A useful study of the Principle of Duality in its more elementary aspects is to be found in René Roy, *De l'Utilité* (Paris Hermann, 1942). M. Roy brings out the analogy (indeed the fundamental identity) of this principle of duality with that arising from tangential co-ordinates and therefore with the principle of duality or reciprocity in projective geometry.

3. I pass to the other issue—that of the " imperfect stability of the dynamic system. Here I am more concerned with Professor Lange than with Dr. Mosak ; for Dr. Mosak compresses most of what he has to say on the subject into one decidedly difficult chapter (VIII), while it is on the other hand the main theme of Professor Lange's book. It will, however, be convenient, before I proceed to examine the contributions of either writer to this issue, to indicate my own present attitude towards it ; for this again is a matter which I would now admit to have been treated in *Value and Capital* not quite satisfactorily. My present position is fairly near to that of Professor Lange (nearer, I think, to his than to Dr. Mosak's) ; but it is not quite the same. I shall try to state the correction which I should myself introduce as briefly as possible.

The crucial questions are those of the effects of a rise in the prices of all goods (including factors) in the same proportion : (*a*) rates of interest remaining the same ; (*b*) consequential changes in rates of interest being allowed for. Since the prices of all goods change in the same proportion, we need not distinguish, for the purposes of this problem, between one good and another ; we can lump them all together,[2] and talk about changes in the *price-level* of current goods. Similarly, in problem (*a*), the constancy of rates of interest enables us to treat money and bonds[3] as a single " commodity ", so that we have only two " commodities " in our system, and all the technical difficulties are reduced to a minimum. There remain some rather formidable difficulties about the assumptions under which we are proceeding, and here it is evident that (in *Value and Capital*) I was not sufficiently careful.

If there is a rise in the price-level, and the rate of interest is constant, it is only possible for there to be a positive substitution effect in favour of future goods, if price-expectations have a less than unity elasticity.[4] If price-expectations have a unity elasticity, there can be no substitution effect. This is what I maintained, and this I think is agreed. And since the demand and supply for " money plus bonds " reflects the demand and supply for future goods, there can be no substitution effect in favour of " money plus bonds ".

What, however, of the income effect ? In principle, there will be an income effect, because the initial holdings of " money plus bonds " by different people will differ; in fact, it may be that some people will begin the week with positive holdings of " money plus bonds ", other people with negative holdings. A rise in the price-level means a fall in the real value of these holdings, and this will affect the distribution of purchasing power within the community. Where my analysis seems to have been defective is that it did not take sufficient trouble with this income effect. (I was too much in love with the simplification which comes from assuming that income effects cancel out when they appear on both sides of the market.)

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[1] A minor criticism of one passage in *Value and Capital*, which is made by Mosak in this part of his work (p. 23 note), does not seem to be justified. A more careful reading would have shown that on p. 44 of my book " money " was not being used in the sense of the numéraire, but in the Marshallian sense of " all commodities other than X or Y " compounded into one commodity according to what Lange calls the " law of composition of goods " (*Value and Capital*, p. 33 ; Lange, p. 103). This is made quite clear on p. 44 itself.

[2] By the " law of composition " : see preceding footnote.

[3] Correctly substituted by Lange (p. 15) for my " securities ".

[4] The concept of elasticity of expectations is usefully generalised by Lange, pp. 20–1 note.

Two cases need to be distinguished. In the one case the only money in our economy is a pure credit money. It is money, not a bond, in the sense that it bears no interest; nevertheless it merely registers a debt from one of the " individuals " (who may be a bank) in the economy to another. In this case the positive and negative holdings of money must have been initially equal, just as the positive and negative holdings of bonds must certainly have been equal. A rise in the price-level must consequently make some " individuals " better off to exactly the same extent as it makes others worse off. If the income effects set up by these two movements are symmetric, the net income effect will be zero. The system is in neutral equilibrium.

We shall return to this case in a moment. For the present let us contrast it with the other. If credit money is not the only sort of money, but there is also some " hard " money of whatever kind (it may be metallic money, or it may be a governmental note issue, fixed on principles which are assumed to lie outside our system)—then the positive holdings of money must more than balance the negative; so that even if income effects are symmetrical, a fall in the real value of money will diminish real purchasing power, and the stability of the system will be maintained by the income effect. Of course, even in this case, the system is not necessarily stable. Its stability may be upset by asymmetric income effects; and it may be that that would be quite likely to happen, once the usual stabilising effect of the substitution effect is removed. Instability through asymmetric income effects is, however, a perfectly general possibility, which runs through static, as well as dynamic, analysis.

So much for the first problem, that of the stability of a system with constant interest rates. It is not surprising to find that when we allow for repercussions on interest rates, we have to maintain the same distinction. We have seen that a pure credit economy, with symmetric income effects, is in neutral equilibrium when rates of interest are held constant. *In such a case it is not very evident why the rate of interest should move, when it is free to move.* For the creation of money, in such an economy, will depend upon willingnesses to lend and borrow, just like the creation of bonds. The maintenance of the same *real* system, at a higher price-level, should give an inducement to a creation of money, sufficient to support the higher price-level.

The system is not merely imperfectly stable; it is in neutral equilibrium even when all repercussions are allowed for.

On the other hand, the economy with some "hard" money is not merely likely to be kept stable by the income effect previously discussed; it is also likely to find that at the higher price-level there is insufficient "hard" money to support this price-level, so that there is a stabilising effect through the rate of interest as well as the stabilising income effect. Thus the analysis given in *Value and Capital* holds for this case, except that the income effect should have been allowed for, and it was (I regret to say) overlooked.

This income effect in the "hard" money case is, of course, the same point as that which has lately been made by Professor Pigou in connection with the Keynesian theory.[1] Even after reading Professor Pigou, as well as our present authors, one may fairly doubt whether the point is of any appreciable importance practically. Nevertheless, there can be no doubt that it is valid theoretically, and in principle ought to be allowed for.

4. I am now in a position to comment on the contributions of Professor Lange and Dr. Mosak to this question of dynamic equilibrium. Dr. Mosak confines himself almost entirely to the case of what I have described above as the "pure credit economy". His analysis seems to be quite consistent in itself, though I do not find that the discussion in terms of what he calls the "elasticity of the expenditure plan" is very convenient. It seems to straddle between income and substitution effects in an awkward manner.

Professor Lange's analysis is more general. Although he does not distinguish formally between the "hard money" case and the "pure credit" case, as I have done above, he proceeds in fact on the basis of the "hard money" assumption; and that, so it seems to me, is the wiser thing to do, because the pure credit economy can easily be treated as a limiting case of the hard money economy, but not vice versa. He takes, however, as his standard case that in which the monetary authority adjusts the supply of the hard money to the real demand for it at each price-level (a new sense of *neutral money*). Thus in the case of unity elasticity of expectations, neutral money corresponds to neutral equilibrium, both the income effect and the interest effect being offset by the adjustment in the supply of money. On both grounds, therefore, there is a presumption in favour of stability if the quantity of money is less adaptable than this, of instability if it is more adaptable.

All this formal analysis is worked out in great detail, very neatly and clearly and (as far as I can see) quite impeccably. But since Professor Lange is writing a whole book on price-flexibility in the dynamic economy (and is not merely treating it as one problem among others, like Dr. Mosak and myself), he goes on to broaden his analysis much further. In the first place he includes a more searching discussion of risk in connection with this problem than had previously been given. In the second place he drops the assumption of perfect competition.

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[1] "The Classical Stationary State" (*E. J.*, 1943); *Lapses from Full Employment*, ch. 5. It would seem to follow from Pigou's analysis that Keynes is not to be acquitted of overlooking the income effect, as Lange (p. 17n.) and Mosak (p. 156n.) appear to think. The crucial part of the General Theory is, of course, the theory of wages, in which wage-rates are treated as flexible.

The chapter on risk is very interesting ; as it results in a complete acceptance of the " representative expectation " device which I used myself as a means of side-stepping the risk problem, I ought to be very pleased with it. But as a matter of fact I have never felt at all happy about this device, and have always hoped to find some way of improving upon it. I still do not feel quite happy, in spite of Professor Lange's work ; though it may be that his arguments are sufficient to permit us to use the device with a good conscience, so far as the particular problem with which he is dealing is concerned.

I had myself surmised[1] that this particular problem of dynamic stability is one where the limitation to perfect competition is easier to overstep than is usually the case in general equilibrium economics. That this is so is shown in detail by Professor Lange. Conditions of monopoly and imperfect competition (in Mrs. Robinson's sense) can be allowed for without making any essential difference to the argument. Professor Lange holds, however, that the case is not quite the same with oligopoly (and oligopsony). His theory of oligopoly is that of the " kinked " demand curve. The new elements of rigidity which this imports into the system are discussed at length, both in the chapter on imperfect competition and in the very important chapter on Innovations which concludes the book. To comment fully upon the latter apocalyptic chapter would require an article to itself ; it cannot be tacked on to the end of this. And I expect that the task had better be left to those who are more expert than myself in the new science of oligopolology.

Unlike Dr. Mosak, who bespatters his text with formulæ, Professor Lange has taken the trouble to clear his mathematics into footnotes. Although, as I have found myself, this abnegation involves risks, it will doubtless widen the circle of his readers. He has, however, added a mathematical appendix (which has also appeared separately as a Cowles Commission paper). A large part of this appendix is concerned with the relation between the stability conditions of the equilibrium analysis used in the text and the stability conditions of the corresponding process analysis worked out by Professor Samuelson. (It would have been interesting if some of the arguments in the text could have been reworked on the Samuelson model.) Professor Lange seems to accept Professor Samuelson's view that equilibrium analysis is merely a special case of process analysis—a process analysis in which lags are absent. I do not myself find it easy to accept this. There is one sense of equilibrium analysis (more or less the traditional sense) according to which it clearly is true that process analysis investigates the convergence (or lack of convergence) of the system towards a position which equilibrium analysis determines. But there is another sense (and it was this sense of temporary equilibrium which I was trying to use in the second half of *Value and Capital*) in which the " process " itself is made up of equilibria—each stage in the " process " is an equilibrium. So it is a case of the hen and the egg. There is a " Principle of Duality " at work here also.

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[1] *Value and Capital*, p. 265, n.2.

Income Tax and Incentive to Effort

By H. S. BOOKER

IN the assessment and collection of income tax there is one principle to which Chancellors of the Exchequer adhere and that relates to the progression of the tax. This principle is that the persons with the higher income in relation to their responsibilities must pay not only a higher tax but also a higher average rate of tax.

Expressed mathematically we should say that the amount of tax (t) is an increasing function of income (x) of a form such that the marginal rate is always greater than the average rate of tax. Thus :—

$$t = f(x)$$

$$\frac{dt}{dx} \text{ is positive.}$$

$$\frac{dt}{dx} > \frac{t}{x}$$

for all cases where tax is payable. (For incomes below the exemption limit $\frac{dt}{dx} = 0 = \frac{t}{x}$ and the principle does not apply.) Ideally too it might be argued that $\frac{d^2t}{dt^2}$ ought also to be positive but practical considerations make it more convenient to have a discontinuous function with ranges in which $\frac{d^2t}{dt^2}$ is zero but such that $\frac{dt}{dx}$ is greater in the range from x_1 to x_2 than in the range from x_0 to x_1 when $x_2 > x_1 > x_0$. Excluding surtax there are now (October, 1945) two such ranges, the first up to £165 of taxable income and the second above £165. In the next financial year it is proposed that there should be three, these being up to £50, £50 to £125 and over £125 of taxable income.

Because of the principle of the progression of tax it follows that when a high average rate of tax has to be imposed it is essential to have the marginal rate very high indeed for it must be higher than the average rate. But this has the disadvantage that it discourages effort to increase his income on the part of the taxpayer. Thus the Exchequer has the task of determining how best to raise a large sum by means of income tax whilst at the same time having little influence upon output. The problem is intensified in times such as the present when the Government is finding it necessary in some ways to relax controls over labour. It is the high marginal rate of tax which makes the worker wonder whether it is worth his while to work overtime or to attempt to increase his earnings at piece-work.[1]

In adjusting income tax in the light of the immediate post-war situation consideration has to be given to the problem of increasing incentive to effort without a very substantial decline in revenue. The October budget has answered the problem by reducing the marginal rate of tax on earned income for those who pay at the standard rate by a very small amount, indeed by only ten per cent., from nine-tenths of ten shillings to nine-tenths of nine shillings in the pound, but by modifications of personal allowances and of the reduced rate of tax has made substantial changes in the marginal rates for limited classes of taxpayers who have had reductions from nine-tenths of six shillings and sixpence to nothing or to nine-tenths of three shillings. It is possibly hoped that these limited classes will be those most influenced in their efforts by the high marginal rates of tax. The modifications proposed in the October budget are estimated to cost £322 million in a year, which is about one-quarter of the present revenue from income tax. The cost of the concessions sounds very expensive indeed. It is indeed difficult to see how incentive to effort can be greatly increased by such modifications in the rates of income tax except by budgeting for considerable losses in tax revenue.

Are we therefore to assume that retaining the principle of progression in income tax is inconsistent with both maintaining a high revenue and with increasing substantially incentive to effort ? It seems quite clear that marginal rate of tax for many people must remain high but it might be well to examine a little further the nature of the marginal rate of tax.

We as economists have been brought up on the idea of marginal analysis and we have found it a very useful concept. But do not let us imagine that other people also think in terms of marginal analysis ; they very rarely do unless something happens to force that line of thought upon their minds. In real life even the business man who is convinced by the analysis finds it difficult to apply because we have not yet found any satisfactory method of measuring marginal values. We find that we cannot construct supply and demand curves which convince one as being accurate. We only know that at a certain time a certain supply is absorbed at a specific price, which simply means that we can plot a point, not a line, on a graph, and a point has no direction or rate of change, that is it has no marginal value. If another point is plotted appropriate to another time it becomes almost impossible to allow for " other things " which have not remained " equal ". It seems likely therefore that to the ordinary man marginal analysis rarely has much significance.

Has this any significance for the Exchequer ? It is probable that it has. Recently something has happened to force the concept of the marginal rate of tax to the thoughts of the taxpayers. That something is the system of pay as you earn income tax. Before the war the taxpayer paid income tax every half-year and he paid on the basis of his income in a preceding year. There was nothing to connect any specific part of his tax with any particular earnings. He probably simply thought in terms of total income, total tax and average tax per unit of income. Much the same thing happened when workers first had income tax deducted by their employers. The deductions were based on an earlier year's income and did not fluctuate according to the current week's income.

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[1] For a fuller discussion see F. W. Paish in ECONOMICA for August, 1941, on " Economic Incentive in Wartime ".

Then P.A.Y.E. was introduced with the system of altering the tax deducted according to the current week's income. If a person was liable to tax at 10s. in the pound he only received 11s. out of every extra pound he earned whether from overtime, piece-work or other effort. He had the idea of marginal tax forcibly brought to his notice and resented it. Had the old system of collection remained in operation even with the high wartime rates of taxation we should probably have heard very much less of the argument " Why should I work overtime when half of my earnings are taken from me ? " As Mr. Paish stated in 1941 (op. cit.), before the introduction of P.A.Y.E., ". . . the deterrent effect of high marginal taxation is probably substantially reduced by the present system of assessment . . ." for the " time-lag . . . probably prevents many taxpayers from realising clearly how much of their marginal earnings goes in taxation ".

It might be concluded that this argument suggests we should go back to the old system of tax collection. But there is an alternative, a compromise between the two systems which might be very useful. It has been pointed out that it is the marginal rate of tax that deters extra effort and also that marginal rate is often high in relation to the average rate. Could not something be done to make the marginal rate equal to the average rate in the short run week by week payments whilst retaining the principle of the progression of tax in the longer periods ?

It might be done. Let the current year's tax be based on a past year's income, but on the current year's rates of tax and allowances, as it was before the war but in a new way. The assessment made on the basis of a man's income in the past year could be used to obtain, not the actual amount of tax he should pay in a subsequent period, but the rate at which he should pay. Thus under the new (October, 1945) proposed rate of income tax a married couple with earned income of £300 a year would pay[1] £19 10s. a year, this being equivalent to an effective average rate of about 1s. 3d. in the pound. The suggestion is that instead of collecting £19 10s. from the earner of that married couple in the year in which he earned it by giving him a code number and his employer a table of deductions appropriate to various earnings we should simply request that employer to subtract 1s. 3d. in the pound from that man's earnings during a subsequent year. In this way his

[1] See note on next page.

marginal rate of tax during the year would be 1s. 3d. in the pound instead of nine-tenths of 6s. 0d. In a similar way if the man's earnings had been £400 in the assessment year his marginal rate of tax would become[1] 2s. 9d. in the pound instead of nine-tenths of 9s. 0d. The following table gives illustrations of the difference between marginal and average rates of tax under the current (October, 1945) rates of tax and those proposed for the next financial year.

MARRIED COUPLES WITHOUT CHILDREN

Income per Year	*Rate of Tax per Pound of Income*			
	Marginal		*Average*	
	Current	*Proposed*	*Current*	*Proposed*
	s. d.	s. d.	s. d.	s. d.
£200 ..	5 10	nil	1 3	nil
£250 ..	5 10	2 8	2 3	6
£300 ..	5 10	5 5	2 10	1 3
£400 ..	9 0	8 1	4 0	2 9
£500 ..	9 0	8 1	5 0	3 9
£1000 ..	9 0	8 1	7 0	6 0

There are, of course, difficulties which would have to be overcome, but none seem greater than those which existed when normally one paid tax on earnings made a year or more previously. There must be provision for adjustments in the case of drastic changes in earnings,

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[1] NOTE :—

	£	£
Income of married couple (earned) ..	300	400
Earned income allowance (one-tenth) ..	30	40
Personal allowance ..	180	180
Total allowances ..	210	220
Pay tax on ..	90	180
Tax to pay :—		
£50 at 3s. ..	7½	
£40 at 6s. ..	12	
£90	19½	
£50 at 3s. ..		7½
£75 at 6s. ..		22½
£55 at 9s. ..		24¾
£180		54¾
Average rate of tax in £ ..	1s. 3d.	2s. 9d.
Marginal rate of tax in £ nine-tenths of ..	6s. 0d.	9s. 0d.

but the importance of this is less than when the whole basis of the tax was earlier earnings. It is now suggested that only the rate should be based on earlier earnings, the actual tax should be based primarily on the current earnings.

Individuals often have " unearned " as well as earned incomes. It might be administratively difficult to tax these at the rate appropriate to the earned income. There seems no reason why it should not be taxed as at present, that is at either 10s. or 6s. 6d., or as presumably will happen after next March at one of the three rates 9s., 6s. or 3s. in the pound.

It might be objected that the new proposal does not fundamentally alter the position with regard to marginal earnings. It might be argued that although the man who works overtime does not lose immediately any substantial portion of the overtime in tax, he does lose it in the long run. This argument is certainly correct ; if the man has a lot of extra earnings in any year his next year's rate of tax will be increased. It has indeed been pointed out earlier that in the long run a higher marginal rate of tax than of average tax is essential to the principle of progression. But psychologically the position under the suggestions made in this paper is very different from that under P.A.Y.E.

A Note on the Elasticity of Expectations

By L. M. Lachmann

I

In recent years the *Elasticity of Expectations* has come to occupy a prominent place in dynamic theory. Economists, baffled by the apparent intractability of expectations, which cannot be treated as determined by other " data " although it is clear that they are susceptible to economic experience,[1] were duly grateful for an analytical tool which enabled them somehow to grapple with the matter. In creating the concept Professor Hicks[2] has provided us with a convenient criterion of classification of the modes of a relationship, which we can use even though we may know nothing about the causes and conditions of the various modes. More recently, Professor Lange, in his admirable investigation of the effects of flexible prices on the volume of output and employment,[3] has made ample use of the concept. Naturally, it has the defects of its virtues. As the most elaborate thermometer can tell us nothing about the causes of the fever from which the patient is suffering, so the elasticity of expectations, being a measure, can tell us nothing about what causes the magnitude of our object of measurement.

But where, then, are we to turn for information about the factors on which the magnitude of the elasticity of expectations depends ? Professor Hicks, it is true, has attempted to find an explanation in " the psychological condition of the individuals trading "[4], in the greater or smaller " sensitivity " with which different individuals react to identical price changes. But this attempt to explain an economic phenomenon by reducing it to one of " group psychology " remains unconvincing, if for no other reason than that it would commit us to the assumption that the same individual, confronted at different times with price changes of equal magnitude, invariably reacts in an identical, and therefore predictable, manner. The causal explanation of the magnitude of the elasticity of expectations has to be sought elsewhere.

It seems to us that it can be found in an empirical fact quite familiar in the literature dealing with expectations that has sprung up during the last ten years, but which, for some strange reason, most authors treat as an obstacle to be overcome rather than as a potential element of their analysis. The fact we have in mind is the well known one that the object of our expectation is rarely one price expected with certainty, but usually a whole set of prices expected with various degrees of probability. Disregarding the most improbable prices we obtain what Professor Lange has called " the practical range ".[1]

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[1] On this and related aspects of the problem cf. J. A. Schumpeter : *Business Cycles*, 1939, Vol. I, pp. 55–6, 140–1.

[2] J. R. Hicks : *Value and Capital*, 1939, p. 205.

[3] Oscar Lange : *Price Flexibility and Employment*, Cowles Commission for Research in Economics, Monograph No. 8, Bloomington, Ind., 1944.

[4] Op. cit., pp. 271–2.

" Entrepreneurs and consumers need not, and usually do not, visualise an exact probability distribution of possible prices. For our purpose it is sufficient to assume that each person forms some idea about the most probable value and the ' practical range ' of the expected price. For instance, an entrepreneur or consumer thinks that the price of some specified good at some given future date will be most probably $100, but in any case not less than $80 and not more than $150. He may believe that there is some slight probability that the price will turn out to be below $80 or above $150, but this is so small as to be negligible in practice and he takes the chances of disregarding such outcomes altogether. Such an assumption seems to be quite realistic."

Curiously enough, like most authors in this field, Professor Lange shows himself eager to discard this valuable discovery as soon as it is made, and the only use made of the " practical range " is as a " measure of the degree of uncertainty of price expectations ". He immediately proceeds to " substitute for the most probable prices actually expected with uncertainty equivalent prices expected with certainty ". In this way the range is compressed to a point, a " certainty-equivalent ".[2] This seems to be widely regarded as a gain, presumably because it is thought to facilitate comparison between actual and expected prices if both are brought to the common denominator of single values. By contrast, we shall try to show that by substituting single-value expectations for the uncertainty range of expected prices we stand to lose more than we gain, because reaction to price change will largely depend on the location of the prices affected within the scale of expected prices.

II

Let us assume that in Professor Lange's example in which expected prices range from $80 to $150, the actual market price was, to start with, $109.[3] Now, let the market price move, either rise above or fall below $109. It is readily seen that as the market price approaches the neighbourhood of the upper or lower limit, say, $140 or $90, expectations will tend to become inelastic. People will think that the price movement " cannot go much farther ", and anticipate a movement in the opposite direction, perhaps after a temporary stagnation ; the narrower the uncertainty range the sooner expectations will become inelastic. On the other hand, as long as the market price moves between, say, $95 and $135, people's expectational reaction will not be affected by the actual movement and expectations will therefore be indifferent ; the wider the range the larger is this " indifference zone ". But as soon as the market price passes either the upper or the lower limit, a new situation arises. People, shocked out of their sense of normality, will have to readjust the basis of their predictions, and in the interval before forming a new, and probably wider, uncertainty range their expectations are likely to become elastic. A narrow range will thus at first, as we saw, cause inelastic expectations at an early stage of the price movement. But if in spite of them the movement continues, expectations will become elastic the sooner the narrower the range.

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[1] Lange, op. cit., p. 30.

[2] " He will, however, be able to select a single value which, if he could expect it with certainty, would have the same significance for him as the distribution of probabilities which he has actually in mind. We shall call such a value a certainty-equivalent." G. L. S. Shackle: *Expectations, Investment and Income*, 1938, p. 64.

[3] There is, of course, no reason why the actual market price should coincide with the median of our probability distribution. The extent to which the former deviates from the latter measures the " bullishness " or " bearishness " of the market. The more " bullish " the market the lower the place on the scale at which we shall find the market price, and the more the latter will fall short of the median. And vice versa.

Two conclusions emerge from this reflection. In the first place, the existence of an uncertainty range gives a new significance to inelastic expectations. We can say that, as a rule, any price movement, if continued long enough, will sooner or later encounter inelastic expectations, because it will sooner or later touch the fringes of the range. Under anything like normal circumstances those inelastic price expectations of which Professor Lange has shown that " given the monetary effect "[1] they " always tend to stabilise the economy ", will finally prevail. Within limits the economic system has a good deal more stability than appears.

Secondly, the degree of elasticity cannot be divorced from the kind of price change the reaction to which it measures. " Explosive " price change is seen to be the main cause of elastic expectations, both in the sense of violent change, and in that it destroys the existing basis of expectations, the sense of normality, which provided a criterion of distinction between the more probable, the less probable, and the highly improbable. It does so by demonstrating that the highly improbable, which had been excluded from our range, is possible after all. Now, as we saw, a price will pass the limits of the range with difficulty. As it approaches them it encounters increasing pressure from inelastic expectations resulting in sales at the upper and purchases at the lower limit. To overcome the pressure of these stabilising market forces the price movement will most probably have to be carried by a strong " exogenous " force, i.e., one originating outside the market, unknown to it and therefore not taken into account when expectations were formed. Changes in the " responsiveness " of the monetary system (only, of course, if they were not expected) are an obvious example.

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[1] Lange, op. cit., p. 27.

An important objection may be raised against our method of analysis on the ground that to assume an uncertainty range invariant with respect to actual price change is to misinterpret the meaning of *Elasticity of Expectations*. The concept involves a relationship between an actual price change and an expected price change accompanying it. Are we, then, entitled to treat the range of expected prices as independent of actual change ? When, in our example, we allow the market price to rise above, or fall below, $109, do we not have to assume a concomitant shifting of the range ? It might even be argued that our conclusion about inelastic expectations in the neighbourhood of the limits follows merely from our assumption of an expectational range invariant with respect to actual change. Our reply to this is that we fail to see any plausible reason why the range should shift at all as long as the actual price moves within its limits, and that therefore the burden of proof is on the other side.

The objection mentioned raises, however, a fundamental issue in the theory of expectations. It vividly illustrates what is in our opinion the main defect of the " elasticity of expectations," viz., the *purely mechanical* treatment of the relationship between actual and expected price change to which its use commits us. It compels us to interpret " a given degree of elasticity " as meaning that every price change will lead to a proportionate shift in the range of expectations, irrespective of whether that price change was formerly regarded as probable, improbable, or whether it came as a complete surprise. That does not seem to us to be a sensible hypothesis, and we suspect that it would never have been made but for the fact that most economists, when they come to deal with the elasticity of expectations, have forgotten all about the uncertainty range and started to think exclusively in terms of a point-to-point comparison.

It seems preferable to base the relationship between actual and expected price change on a clear distinction between price phenomena which are consistent with the existing structure of expectations, fall " within the range ", and thus cause no disappointment—and phenomena inconsistent with the structure of expectations, which fall " outside the range " a revision of which they necessitate. This vital distinction is apt to be concealed by the way in which the " elasticity of expectations " is usually handled, even by so brilliant a performer as Professor Lange.

III

The reader might think it less than fair if we concluded these few critical remarks without at least giving an indication of a more positive point of view. We have criticised what we called the *purely mechanical* treatment of the relationship between actual and expected price change. By this we mean the treatment of the relationship as a mere " reaction equation ", which leaves no room for the *interpretation* of phenomena observed, an activity of the mind from which the relationship after all derives its meaning.[1] But what kind of treatment are we to substitute for it? We believe that progress in the theory of expectations is most likely to be made along lines like those to be described in what follows.

The formation of expectations is always incidental to the diagnosis of the situation in which we find ourselves; no prognosis without diagnosis. Such a diagnosis necessarily runs in terms of an analysis of forces. That we expect a certain event to take place at a future date T_1 means that after a survey of what we regard as the major forces (obstacles included) likely to be operative in the period between "now" and T_1 we have arrived at the conclusion that their combined net result will be the event at T_1. An expectation always implies a judgment on the character and strength of the economic forces producing change, believed to be major forces and operative in a situation. The *subjective* nature of expectations, due, in the first place, to divergences in individual interpretations of identical observable events, is thus seen to derive ultimately from divergent judgments on the strength of the economic forces believed to have caused them.

Now, expectations may be disappointed. When T_1 becomes the present, our event may fail to happen, or an altogether different event may take place unexpectedly. We know then that our original diagnosis of the situation was wrong, that we had incorrectly assessed the strength of the various forces, or may have altogether ignored a major force, and we shall revise our diagnosis retrospectively.

In applying this reasoning to our uncertainty range we find that it offers a convenient framework for the testing of our diagnoses. A range extending from $80 to $150 means that people think they know enough of the nature and strength of the forces operative in their situation to allow them to predict that the price will be neither above $150 nor below $80. The width of the range expresses the degree of our uncertainty about the exhaustiveness of the information at our disposal. If we thought we knew everything relevant to the expected event about the forces, major and minor, which shape the situation, we could predict one price with certainty. An increasing range expresses an increasing uncertainty about the completeness of our knowledge.

The next point to grasp is that any price movement taking place between "now" and T_1, i.e., between the date at which the prognosis is made and the date to which it refers, can be regarded as a test of the diagnosis which forms the basis of our prognosis, throwing additional light on the nature and strength of the forces surveyed, and thus as adding to our information. As long as the price movement is confined to within the range, it does not provide relevant new information, but merely confirms the soundness of the diagnosis which found its expression in our range. That is why we said above that as long as the price moves within the middle reaches of the range, people's expectational reactions will not be affected by the actual movement and expectations will therefore be indifferent. But as soon as the price moves beyond the limits of the range, the inadequacy of the diagnosis on which the range was based becomes patent. A new situation has arisen which requires a new diagnosis.

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[1] On the significance of the interpretation of observable phenomena in the theory of expectations cf. L. M. Lachmann: "The Rôle of Expectations in Economics as a Social Science," ECONOMICA, February, 1943, esp. pp. 15–18.

We submit that if the objection mentioned above is valid, so that the elasticity of expectations has to be construed as implying that, with a given degree of elasticity, every actual price change, however people may interpret it, results in a corresponding shifting of the range, the concept would be deprived of all value as an analytical device. We should then reluctantly have to conclude that a formulation of the relationship between actual and expected price change, which refuses to seek the causes of expectational reactions in men's *interpretation* of observed phenomena, and compels us to substitute mechanical hypotheses for the logical analysis of a meaningful pattern of human action, cannot be regarded as a useful tool of economic analysis.

Book Reviews

Food for the World. Edited by THEODORE W. SCHULTZ. University of Chicago Press. 1945. xiv + 343 pp.

The papers given at the meeting of the Norman Wait Harris Foundation in Chicago in September, 1944, are assembled in this book, together with part at least of the discussions on them. The organisers of the Conference, as Professor Schultz states in the introduction to the book, sought to collect the ideas and analyses necessary to understand the rôle of food in world affairs and to bring out the fundamental elements appropriate to a food policy in a world setting. With this aim they obtained papers from students, mostly American students, of population, nutrition, food supplies and international relations, and from some administrators dealing with these subjects. The field covered is thus necessarily wide.

Proceedings of conferences rarely make good books. Many readers will find that a large number of the papers printed here fall between two stools. Almost inevitably they assume too much previous knowledge, either of facts or of current controversy, for those not already expert in the particular aspect of the subject dealt with, but are not sufficiently novel or worked out in enough detail to contribute a great deal to experts. Some of the essays, however, successfully avoid these difficulties.

Only a few of the papers can be referred to in the space of a review.

Part I of the book consists of two papers on the development of food policy before the war. Dr. Boudreau's essay is largely a summary of the Final Report of the Mixed Committee of the League of Nations on " The Relation of Nutrition to Health, Agriculture and Economic Policy ", and a plea for the establishment of advisory committees in future. Professor Black provides a useful summary and analysis of U.S. food and agricultural policy, largely in the '30s.

In Part II, on population, Dr. Notestein's interesting paper studies past population movements and estimates population in the year 2000 in the different continents and for some of the more important countries or groups of countries. He distinguishes between countries of three population types—incipient decline (e.g., U.S.A.), transitional growth (e.g., Russia), and high growth potential (e.g., India). He estimates world population in the year 2000 at over 3,000 million, but points out that such a low figure can only be justified if in Asia economic productivity increase either insufficiently to reduce the present high death-rate, or so rapidly as to permit rapid modernisation, the prerequisite for a fall in the birth-rate.

In Part III the economist will probably be most interested in Professor Maynard's qualitative paper on the Knowns and Unknowns about What Constitutes an Adequate Diet. The remaining papers are more specialist or more obvious.

Professor Wilcox's paper in Part IV provides a useful analysis, product by product, of the probable movement in food supplies in Western countries after the post-war transition period. He expects a large increase in the output of fats, animal proteins and vegetables, and a possible re-appearance of market surpluses; but he does not think such surpluses need be alarming, provided that reasonably full employment can be maintained and a start made in lowering trade barriers. Dr. Tolley contributes a wise paper on Agricultural Adjustment and Nutrition, which includes the calculations made by the U.S. Bureau of Agricultural Economics on the probable consumption and relative farm prices in 1950 of a number of major agricultural products.

Part V, on International Relations, is mostly written in too general terms to convince those in need of conviction. But all those responsible for organising international bodies or conferences should read Dr. Henry C. Taylor's brief but trenchant analysis of the Major Obstacles in the Path of International Collaboration in Food and Agriculture, derived from his long experience.

In Part VI Professor Schultz provides a useful though not very new analysis of the probable trend of production and prices in food and agriculture and shows that in due course resources will need to be diverted from agriculture to other occupations. Dr. Brandt concludes with an essay directed against most forms of state interference with food or agriculture—an essay which is polemical rather than argumentative; in the discussion which follows he is rightly but unsuccessfully pressed to clarify and justify his views.

The organisers of the conference hoped that it would examine divergencies of view on policy, evaluate their respective merits, and discover whether or not they could be reconciled. The time at its disposal to digest the undoubted divergencies that emerged does not seem to have been sufficient to achieve at all fully the two latter aims.

Ruth Cohen.

Bank Liquidity and the War. By Charles R. Whittlesey. National Bureau of Economic Research. May, 1945. v + 86 pp. 50 c.

The impact of the Second World War upon the American banking system has been in many respects similar to that experienced in this country. An embarrassing flood of liquidity has largely replaced the banks' customary assets by Government obligations, so that it is possible for Professor Whittlesey to say, almost echoing Mr. Crowther's summing-up of corresponding changes in Great Britain : " Banks have now become chiefly custodians of Federal debt, instead of being concerned mainly with furnishing working capital for commerce and industry ". Between April, 1942, and December, 1944, the Federal Reserve banks absorbed nearly $12,000 million of Treasury Bills, and the commercial banks nearly $16,000 million of Treasury certificates of indebtedness.

So intensive has been the drive to convert idle resources into Federal debt that the ratio of cash plus interbank assets to deposits in member banks fell from 42.2 per cent. in June, 1940, to 23.5 per cent. in June, 1944. In the same period, short-term Government debt rose from 1.5 per cent. to 18.5 per cent. This has been rendered possible by the elaborate inducements offered by the Federal Reserve system, including in particular the use of open-market operations to maintain a fixed pattern of interest rates on Treasury obligations, and a standing offer of loans on any class of Government security at rates of interest lower than those which the securities earn.

In the process, some interesting developments in the whole system have taken place. The number of banks has dwindled, and their average size has doubled since June, 1939; the increase has been greatest for country banks, and least for Central Reserve city banks, which is just the opposite of what happened between 1920 and 1939. The total of sub-standard assets fell from nearly $3,000 million in 1939 to $1,250 million in 1943; and the average maturity date for all insured banks' assets, which had been 4¼ years ahead in June, 1939, was only 3½ years distant in June, 1944.

Most interesting development of all, excess reserves have largely disappeared, except in some country banks, having been replaced principally by Federal securities. This has involved some resumption of borrowing from the System at peak periods, but the reluctance of member banks to borrow appears to have been overcome by the special inducements referred to above. The exclusion of country banks from the field over which excess reserves has largely vanished is no doubt partly accounted for, as Professor Whittlesey suggests, by a greater conservatism, and partly by a feeling that the net income earned, after deducting E.P.T., is not worth the bother. It is, however, also conceivable that the country banks regard these reserves as a partial insurance against a reversal after the War of the recent trend of deposit-growth. This appears to conflict with the evidence, in the scattered diagrams on p. 26, of the absence of any correlation between deposit-growth and liquidity-preference; but the explanation may be found in the fact that these diagrams group the banks geographically, and not by size.

Professor Whittlesey has given us a compact and stimulating study of a large subject. His pamphlet, which is well furnished with statistics, brings out trends which may easily be overlooked in more general studies, and is at the same time a useful contribution to the study of the perennial problem of bank liquidity.

J. K. Horsefield.

International Currency Experience. League of Nations, Economic and Financial Series. 1944. 249 pp. 12s. 6d.

Even more than the Bank Restriction Period, the twenty years between the Wars are likely to attract the study of economic historians. Few periods in world history stand out with equal sharpness from the epochs which preceded and followed them. Had they been years of placidity and conservatism few could fail to be attracted by their unique determinateness; the specimen is pinned for examination in a way which rarely happens in the social sciences. Since they were, in fact, years of tumultuous strain and of intensive development, alike in industrial technique, political experiments, and economic theory, they offer an irresistible bait to anyone with an itch for research.

Few, however, are better qualified to attempt to explain the medley than the writers of this League of Nations publication, of which (to quote the preface) " the greater part . . . is the work of Mr. Ragnar Nurkse; but Chapter VI was written by Professor William Adams Brown, Jr.". It is not clear from this discreet ambiguity whether others have also collaborated, but all concerned are to be congratulated on an exceedingly useful compilation.

It consists in effect of a series of studies on different aspects of the struggle to solve the problem of the relationship between the mechanisms for stabilising international exchanges and the newly-developing urge to seek economic stabilisation at home. The chapters deal primarily with the Gold Exchange Standard, the Sterling Area, the problems of International Currency Reserves, exchange fluctuations, exchange control, and the influence of changing environments. On all these topics the book's large common sense throws valuable light; but its chief contribution is the success with which it relates transitory phenomena to the main issue. It is this admirable sense of proportion which enables the authors to deal freshly with such familiar subjects as the doubtful value of devaluation, the difficulty of passing judgment on policies of neutralising gold flows, the real significance of the success of the sterling area, and a dozen others. Mr. Nurkse (if it is correct to attribute to him the final chapter of " Review and Conclusion ") has an enviable faculty for crystallising in a lucid and compelling phrase the syntheses after which most of us are still unsuccessfully groping.

The standpoint from which the book is written is that the older conceptions of monetary responsibilities—as comprised, for example, in the " rules " of the Gold Standard " game "—fought a losing battle throughout the period against the newer ideas which first found an authoritative utterance in the " General Theory ". The changing rôle of international currency reserves is thus treated as comprising a transformation from their traditional function of " transmitters " of economic fluctuations to that of " buffers " against the importation of depression. From this change flowed the development of import restrictions and of exchange adjustments, exchange control and exchange rationing. But these are clumsy and inefficient tools to achieve improvements in domestic incomes, especially when compared with direct domestic expansion. Moreover, such policies have a habit of spilling over from the negative functions of mere " buffers " to positive activities of a " beggar-my-neighbour " kind, provoking retaliation in kind, and obviously wholly unjustifiable. Hence the only real remedy for international disequilibrium is an all-round agreement to maintain domestic policies which will maximise effective demand—a conclusion which paradoxically resembles its own complete antithesis, the " defeatist policy " (as the book calls it) of *sauve qui peut* in international relations.

Any teacher needing a succinct guide to the multifarious problems which this book discusses will find here a *vade mecum* of a particularly satisfying kind.

J. K. HORSEFIELD.

Federal Finance in Peace and War with Special Reference to the United States and British Commonwealth. By G. FINDLAY SHIRRAS. Macmillan & Co., Ltd. 1944. 377 pp. 21s.

The system of governmental finance in countries with a federal constitution is a topic which hitherto has been almost unknown to British readers, and the present volume will do as much as a single book can to remedy this defect.

The book is primarily statistical and descriptive. In it Mr. Shirras deals with the fiscal systems of the United States, Canada, Australia, South Africa and India at various dates since about 1910 but mainly during the period 1937–43. In Part I, the longer portion of the book, a separate chapter is devoted to the analysis of the finance of each of these countries. Part II sets out some of the distinctive problems which arise in financing government in a country with a federal constitution and Part III consists of tables of statistics.

Introducing the account of each system is a rapid sketch of the main features of the country's economy, and, since the constitution prescribes the limits within which the fiscal system must move, there is an outline of its constitutional development. The discussion concludes with a view in some detail of the response of the different "layers" of government, down to 1943, to the need for revenue brought by the war. The core of the discussion, however, is a statistical scheme round which the author's treatment centres: by the aid of a few tables of particular types he has been able to lay bare many of the essential features of the fiscal system of each country. This is true at any rate of the country's taxation and expenditure, though less true of its public debt. Also, by considering the main variables in relation to national income, a sense of perspective and some degree of comparability between country and country is obtained.

The types of table which the author employs would be difficult to improve upon: they have the utmost clarity and have been designed to bring out upon which classes of the community the tax burden rests and which classes benefit from governmental expenditure. The individual taxes are listed and arranged in the convenient groups, progressive taxes, taxes on property, on business and on consumption expenditure: cross headings show whether a tax is levied by the federal government, the state governments or the local authorities. The various items of expenditure are shown and are arranged in the groups, expenditure for defence, maintenance of justice, etc., for social services and for development (e.g., railways and roads). In the case of public debts, where the debts have been contracted for particular purposes this information is utilised; but when a debt cannot be attributed to a particular purpose the type of classification most promising to the economist is defeated. By the circumstances of the case Shirras' treatment of debts is forced to vary between country and country and is much less enlightening than his treatment of taxation and expenditure.

That the whole exposition is highly successful is due in part to certain identities and contrasts running through the book. The same simple scheme of analysis is applied to the complex material of fiscal systems so different as those of the United States, South Africa and India; and again the various countries in the consecutive chapters are seen finding their own adjustments, very different as between one country and another, to the same need for revenue created by the war. The mode of analysis gets emphasis from repetition; and the cases dealt with are so different that each can be clearly marked off from the others and finds its place in a series by differentiation from them.

In Part II the author indicates the main methods of co-ordinating revenues and expenditures in federal countries, namely, by attaching the right to use a particular tax to either federal or state government, by sharing the revenue from a tax in some definite proportion between the federal and other governments and by grants-in-aid to the states. The manner in which the increased need for revenue has been met during the war is used in a discussion of the possibilities of financing increased expenditures after the war with a view to securing full employment.

Part III consists of eighty pages of tables of statistics. This enables more detailed information to be presented on a number of topics and information to be given which is important for some countries but not for others and which would not have fitted into the uniform scheme of the text.

Mr. Shirras has long been interested in the problem with which this book deals, and by dint of much research has been able to collect the materials which show the problem as it exists in its various phases at the present day. Rich materials are presented and the argument around them is always clearly developed. Even the economist who is interested in the finances of only one of the countries dealt with will find guidance here such as will be hard to get anywhere else; and Mr. Shirras' book may lead him to understand the fiscal system of the country much more fully by letting him see it as a particular case, carefully classified and described, in the species to which it belongs.

D. BLACK.

Economic Stability in the Post-War World. Part II of the Report of the League of Nations Delegation on Economic Depressions. 1945. 319 pp. 10s.

The first part of this report, published in 1943, dealt admirably with the economic problems of the post-war transition period. It was clear, concise, readable and—

above all—had the great merit of distinguishing between the general and fundamental, as contrasted with the special and technical, problems involved. Part II deals with the long-term problem, i.e., " the conditions of prosperity after the transition from war to peace ", with particular reference to the urgent need for, and the methods of securing, economic stability. Its main theme is that it is as feasible for countries to adopt anti-depression policies from which all will benefit, if adopted in unison, as it is for them to adopt policies which, when generalised, injure all (as occurred after the last war). After analysing the problem and various types of remedial policies, the report proceeds to propound the means by which the errors of the inter-war period can be avoided.

Although there is little with which one can disagree in its analysis of the nature of the problem (which follows what has now become the orthodox Keynesian view), this report is, on the whole, a disappointing document. To begin with it falls between two stools. It is too lengthy for the general reader, but contains little, if anything, novel for the economist. The authors themselves refer to their consciousness of how much space they have devoted to recommendations directed almost wholly to national authorities, although they hold that it is essential to formulate contracyclical economic policies within a frame that is international in scope. Much has already been published on the subject of National Full Employment policies and this report is for the greater part merely retreading a well-worn path in this sphere. On the other hand, little work has so far been done on the international aspect, and it is here that there is most need for clear exposition and discussion. One cannot help feeling, therefore, that the authors would have served their purpose better by very greatly condensing the first two sections of their report (especially section II) and thus giving greater prominence to their discussion of the international framework and of the various methods of securing concerted and co-ordinated international action to prevent depressions and assure expansion. As it is, by far the most valuable chapters are those dealing specifically with the international aspect. Chapter XVII, for instance, contains an excellent discussion of the measures that may be necessary to counteract deflationary influences from abroad (a subject to which Sir William Beveridge merely gave a passing reference), whilst Chapter XIX deals with anti-depression measures especially appropriate for raw material and food producing countries.

The second main criticism is that the policies recommended, especially in the sphere of national action, are almost certainly quite inadequate. The proposals hardly go as far as those contained in the British Government's White Paper, and fall far short of Sir William Beveridge's recommendations. No attempt is made to tackle certain difficult but essential questions, such as how best to stimulate and guide private investment, or how to secure the support of organised labour and thus bring about modifications in accepted Trade Union practice, without which the danger will always be present that a condition of full employment may develop into one of galloping inflation.

In conclusion I can only express regret that an opportunity has been missed of presenting in compact form the essential nature of the problem of the trade cycle, and the choice of policies with which we are confronted; and advise the reader to skip quickly over the first sixteen chapters, but on no account to miss the last five.

VERA ANSTEY.

Finance Under Provincial Autonomy. By C. N. VAKIL and M. H. PATEL. Longmans, Green & Co. 1940. 194 pp. Rs. 4.

The short pre-war experiment in Provincial Autonomy in India deserves to be fully considered before estimates are made of the post-war financial outlook in India and decisions are taken as to the division of financial authority and revenue resources between the Central and Provincial Authorities. This volume provides a summary of the financial situation before and after the introduction of Provincial Autonomy, and a useful analysis of the financial policies pursued in the various Provinces. Particular attention is paid to the problem of Provincal debt, and to the nature, effects and potentialities of the Sales Tax. In so far as it provides an outline of the general financial situation, of the problems encountered in the Provinces, and of the varied policies pursued, it deserves a warm welcome. But the account given of the policies pursued in each Province is too sketchy to enable any judgment to be formed of the extent to which the Provincial Governments were ready and able to provide a solution for the financial problems which, for many decades, had baffled the Government of India. The most that can be said is that the general trend of the financial reforms in the Provinces was in the direction of (*a*) readjusting the tax-structure in favour of the poorer classes ; and (*b*) expanding the social services. The question is whether this policy could be expanded substantially, so as to make some real contribution to the problem of poverty in India. The reprint, as Chapter II, of a sketchy and in some respects misleading article from the Journal of the University of Bombay, does not enhance the unity and coherence of the volume. There is still need for a full-length study of the financial work of the Provincial Governments between 1937 and 1939.

VERA ANSTEY.

The Growth of German Historicism. By F. ENGEL-JANOSI. The Johns Hopkins University Studies in Historical and Political Science, Series LXII, Number 2, Baltimore. The Johns Hopkins Press. 1944. 101 pp. $1.25.

This is an interesting and valuable contribution to the history of historicism and historiography, although it is perhaps not precisely what the title promises. For by " historicism " the author understands an attitude or philosophy which is " centred around history ", which looks upon " most spheres of intellectual life as permeated by history ", and which makes history " the *magistra*, if not of active life, at least, to a great extent, of theoretical life " (p. 13); and we need perhaps not expect that a historical study of the growth of this philosophy confines itself to its influence on one small section of intellectual life—historiography. Historicism, as the author defines it (and I have no quarrel with his definition since I have used the term in a similar sense) has an importance far beyond the problems of historiography. Its ideas, for example, of historical " development " and historical " success " (p. 15) have been at least as influential (and, in my opinion, as devastating) in other fields of thought, such as in economics, politics, and ethics. Since these are not mentioned, the book may be best described as a sketch of the growth of historical thought in German historiography in the 19th century.

But there is another important limitation. Any discussion of the various approaches to historiography—for example, of the historicist approach—must raise questions of the method or of the " philosophy " of history; questions, for example, concerning the principles underlying the selection of the problems to be discussed; concerning the methods of solving these problems; concerning the mode of presentation, etc. Now the present study sketches the history of certain views on these questions of the method of history, and as such it is very valuable. But although the book makes a real contribution to the history of historiography, it does not make, in my opinion, a contribution to the problem of the methods of history, or to its philosophy; and more particularly, it does not contribute to the discussion of the merits or demerits of historicism. This must be said in view of the expectations raised by the otherwise excellent *Foreword* by W. F. Albright, who stresses the need for a discussion of these problems, and claims that the book helps to fill " a gap in the fields of historiography and philosophy " (a claim for which the author of the book, however, should not be held responsible).

The book consists of an Introduction followed by ten short chapters, one each on Herder, W. von Humboldt, Goethe, Niebuhr, The Romantic School, Hegel, Ranke, Marx, Schopenhauer, and Burckhart; also, it contains two Chronological Tables. These ten chapters contain useful and interesting information attractively presented in a short space. The one that deals with a topic which concerns economists and, at the same time, in my opinion the best of them, is the chapter on Marx. (The chapter with which I disagree most strongly is that on Hegel.) The way in which this chapter on Marx, in eight pages, develops the theme " History is at the core of Marxist philosophy " (p. 63) is excellent. Marx's great contributions to historiography are fully recognised: " as a heuristic principle, the economic interpretation of history has an importance that hardly can be overrated " (p. 68). The critical remarks, however, are much weaker than the exposition of Marx's historicist assumptions. Thus it is not very pertinent to point out that Marx ought to have explained (but failed to do so) " why just these phases had to come about, in just their actual chronological order—why, for example, feudal forms had to evolve from those of antiquity " (p. 66): surely, this failure is shared by everybody else, and it is at least doubtful whether Marx, from the point of view of his peculiar assumptions and claims, was under any special obligation to solve such a problem. Similarly, the remark that Marx failed to make clear how class antagonism "emerged from an initial classless period of social life" (*ibid.*) hardly raises a serious difficulty. On the other hand the author mentions, without criticism, that "Marx goes to great lengths to prove that . . . class antagonism will eventually come to an end" (*ibid.*); but this, surely, is a point which is much more deserving of criticism since it is a more characteristic and politically much more important part of Marxian doctrine, and since, moreover, the doctrine of "advancement towards a firmly and definitely established common goal" (p. 15) is considered by the author as a fairly typical ingredient of historicism.

With these remarks I have touched upon a serious weakness in an otherwise very welcome book. It is a weakness in matters of principle. The book is written as a history of a certain development in historiography; but it is *not a critical history*. It is, rather, an attempt to understand and arouse interest in the events of the past as they "really happened" (Ranke: cp. pp. 25 and 61). The author adopts, with the exception of a few critical comments, a typical historian's attitude of detached reserve, and he neither explicitly criticises nor accepts the fundamental ideas of historicism.

Now the question whether the adoption of such an attitude—in its extreme form it is "the longing to extinguish his self in order better to penetrate his object" (p. 27)—is generally right or wrong for a historian, shall not be discussed here (especially since I have severely criticised this attitude elsewhere). But even if the adoption of such a detached attitude were, in most cases, advisable for a historian, it cannot possibly be appropriate in the present case, where the subject matter of his investigation happens to be the evolution of certain aspects of *historical method.* For in such a case, a historian cannot avoid taking sides since he has to adopt certain methods in his practical procedure. But as he has to take sides, it will be better if he does so explicitly by clearly stating where he stands. If he continues to uphold his attitude of historical detachment even in a case where it becomes stereotyped and empty, then this may be taken as indicating that he has adopted his practical methods without sufficient deliberation, and that his penetration of the problems of historical method is somewhat limited. But in this case he will hardly be able to give a critical analysis of the history of such problems of method; he will fail wherever he ventures beyond a description of views and attitudes, and especially in his critical comments.

Such, indeed, seems to be the case with the present book; and this is why it fails, in the opinion of the reviewer, to contribute to the problems of the method of historiography, or to the philosophy of history. It gives the impression, rather, of originating out of an attempt to become acquainted with the problems of historiography by way of studying their history—a perfectly legitimate way, and very appropriate for a historian. But this way does not lead very far. It may be an excellent preparation for an attack on the problems themselves, but it cannot replace such an attack. Indeed, the assumption that a study of the history of the problems can replace, or be equivalent to, a study of the problems themselves would mean the adoption of some of the fundamental prejudices of Hegelian historicism.

Is the author a historicist? I do not think that he consciously adopts historicist ideas and methods. But if one analyses his few scattered remarks on matters of method as well as his expressions of criticism and approval, then one cannot avoid the impression that he is more deeply imbued with historicist ideas than he seems to realise. He seems to believe that historicism is 'dated', i.e., that it was a nineteenth century phenomenon whose "very basis . . . came to an end" (p. 17) with Nietzsche's *Vom Nutzen und Nachteil der Historie fuer das Leben.* (I do not agree, by the way, with the author's interpretation of Nietzsche who, I believe, never ceased to be a historicist: 'futurism' is always a version of historicism.) But the very idea that doctrines are in this sense 'dated' is a historicist doctrine—indeed a nineteenth century doctrine which, however, is unfortunately still very much alive. Other historicist doctrines which the author seems to adopt are: the unanalysed, naïve acceptance of the existence of historical collectives (the author speaks of "individuals", using a Germanism analysed by Professor Hayek in this Journal, Vol. X, p. 57) such as nations, and especially historical periods; the doctrine that history has to grasp the unique character ("individuality") of "a person, a nation or a period" (p. 67); the doctrine of "significance and greatness" (p. 72) as selective principles in history; the acceptance of the existence of a "historical world" (p. 68) or of a "process of human history "—i.e., of a broad stream of development whose " flow and surge " and whose " powerful rhythm " (p. 43) the historian tries " to understand, to evaluate and to enjoy " (p. 17); and, perhaps most characteristic of all, the related doctrine of the wholeness or " fullness " (p. 68) of this " historical world ", a doctrine which the author uses in criticising Marx's economic interpretation. (This criticism appears to me even more objectionable than the doctrine attacked.)

It is a consequence of the author's limitation to historiography that his useful second Chronological Table does not mention, for example, the founders of the historical School of German Political Economy, Roscher, Knies, and Hildebrand, even though the first, at least, did not only write on the theory of historiography but was also a historian himself. Herbart and Lotze are also missing. The English Table neither contains any of the historicist economists of the period (such as Mill or perhaps Hodgskin) nor any of the sociologists or philosophers (such as Spencer or the Hegelians). But these are minor points; and the criticism offered here does not invalidate the claim that the book can be really enjoyed as a sincere and able contribution to the history of certain historicist views; views whose influence on our own time—in my opinion a thoroughly bad influence—can hardly be overrated.

K. R. Popper.

British Colonial Theories 1570—1850. By Klaus E. Knorr, Stanford University, California. With a Foreword by H. A. Innis. The University of Toronto Press. 1944. xix + 429 pp.

In the author's own opening words, " the purpose of this study is to present and examine significant colonial theories on the advantages and disadvantages resulting to the mother country from the establishment and maintenance of oversea colonies".

Actually the scope of the book is wider than is suggested by this sentence, for " theories " is not interpreted in the strictly scientific sense, but more broadly so as to include any kind of opinion held by political writers and active politicians on the value or drawbacks of oversea possessions. There is also a brief description of the policies influenced by these opinions. Moreover, the attribute " colonial " is not quite correct, since India is among the territories to which the study refers.

The author has chosen a subject on which, as shown by his numerous quotations and footnotes, an extensive literature is available. It is therefore almost inevitable that the book adds little to knowledge on any particular point. Nevertheless, it represents a contribution of undoubted value to the literature on colonial history in the skilful and conscientious manner in which a vast and scattered material has been handled and in the lucid and lively presentation of the results.

The main theme of the book is the Old Colonial System, as viewed by its advocates as well as by its critics. Part I shows the System in its heyday, while Part II deals with the ideas and material forces which led to its downfall. Since the System formed an essential part of a more comprehensive system of economic policy, the economic reasoning on oversea dependencies supplies the main material for Part I; and this concentration on economic thought and policy gives to this Part a unity and balance which are somewhat lacking in Part II. This is, no doubt, partly due to the nature of the subject, for not only did it happen that in the later decades of the period under review the oversea Empire was made up of countries much more varied in character, but other besides economic considerations became of increasing influence in shaping the outlook on imperial problems.

In particular, the book does not bring out with sufficient clarity the fact that very different sets of problems had to be faced according to whether a dependency was peopled mainly by white settlers, or inhabited predominantly by indigenous and mostly backward coloured races. It is true that this outstanding difference became a determining factor in British Empire policy only gradually and mainly after 1850, but it certainly was not without significance in the first half of the nineteenth century, especially if the subject is viewed not exclusively from an economic angle. The chapter devoted to the abolition of the slave trade and of slavery and to the treatment of aboriginal peoples does not do justice either to the subject or to the literature existing on it. There is, on the other hand, a very informative and interesting chapter on " The Theories on Emigration and Colonization: 1815 to 1850 ".

As a whole, the book is a most welcome addition to colonial literature. The student of modern developments in colonial policy in particular is indebted to Dr. Knorr for his lucid and penetrating analysis of the leading ideas which underlay the Old Colonial System. It will greatly help in the task of discerning in what respects the resuscitation of mercantilist ideas and policies, which we have been witnessing for some time, follows the lines of the Old System, and in what respects it presents novel features.

CHARLOTTE LEUBUSCHER.

Demographic Studies of Selected Areas of Rapid Growth. Proceedings of the Round Table on Population Problems. 22nd Annual Conference of the Milbank Memorial Fund: April 12–13, 1944. Milbank Memorial Fund. New York, 1944. 158 pp. $1.00.

In recent years the discussion of demographic problems in this country has been centred on questions connected with the impending decline of populations in Western Europe and America. This was partly due to the fact that interest was more pronounced in problems nearer home, though the availability of relatively accurate population statistics in countries with a low reproduction rate has probably been an important contributory factor. American demographers, however, have shown a great deal of interest in countries whose populations are still vigorously reproducing themselves and where the pressure of population on means of subsistence is great. In 1944 the Milbank Memorial Fund devoted its annual conference entirely to the discussion of these topics and the present book consists of a collection of papers read at the Conference and reprinted from the pages of the *Milbank Memorial Fund Quarterly*.

The essays deal with the situation in Japan, India, the Near East, Egypt and South Eastern Europe. We are warned that the individual papers remain the preliminary formulations of their authors, and it would indeed be difficult to do more in the short space allotted.

The majority of the authors have to overcome the difficulties set by an insufficiency of reliable data regarding population growth. Mr. Moore, who deals with South Eastern Europe, is alone in confining his analysis to the relation between density of population and agricultural productivity. Apart from two other papers which deal with policy, the remaining authors describe the main factors of the demographic situation in the area which they study.

Perhaps the most interesting of these essays is that of Dr. C. V. Kiser, which deals with the demographic position in Egypt. A very careful outline of the structure and vital indices of the Eygptian population is provided in this paper, and an interesting method for constructing a life table by differencing successive censuses is outlined. From this it is deduced that the expectation of life at birth during the decennium 1927—1937 amounted to about 30 years and a diagram shows that the survivorship function in the Egyptian life-tables lies below the already low values for India for all ages up to 75. Estimates of gross and net reproduction rates for 1937 come to 3.11 and 1.44 respectively, the extraordinary size of the gap being indicative of the very heavy rate of mortality.

Mr. Jurkat breaks new ground in his paper dealing with the position in Arabia and the Near East, but he is more handicapped even than his colleagues by the absence of reliable data, and his results are not very conclusive. The two papers on India and Japan are less original and in the main confirm what has already been known about these countries.

The final two essays are concerned with questions of demographic policy. Dr. Cleland presents a plan for a population policy in Egypt which when first delivered in Cairo greatly shocked the Egyptian government experts in the audience. His suggestions, though sound theoretically, would probably prove unacceptable in practice, given the present situation. Dr. Notestein addresses himself to the more general problem of pressure areas. He points out that areas of heavy population growth are matters which concern America greatly, and that it should be the aim of American policy (and of that of the other leading powers) to foster a reduction of growth potentialities. There is, however, no quick or easy solution to the problem. It will be necessary to establish a type of society in which fertility can be brought under control.

A perusal of these preliminary results makes one look forward with interest to the publication of the more detailed studies which are promised in the text.

E. GREBENIK.

Model Bilateral Conventions for the Prevention of International Double Taxation and Fiscal Evasion. League of Nations Fiscal Committee: Second Regional Tax Conference, Mexico, July, 1943. League of Nations Publications, 1945. II. A.3. 86 pp. 3s. 6d. Date of publication: May 1st, 1945.

This report comes at an opportune moment, following, as it does, the recent conclusion of a Double Taxation Treaty between this country and the United States and the announcement by the then Chancellor of the Exchequer that the conclusion of similar agreements was under consideration.

Double taxation, especially in the field of income taxation, represents a substantial obstacle to international trade and the international movement of capital, an obstacle which has increased in importance with the rapid rise in the level of taxation during the war. We in this country, dependent as we are on our export trade for the maintenance of our standard of living, have an especial interest in the freedom of international trade and in any steps which can be taken to remove the obstacles which stand in the way of its development.

Double taxation owes its origin in the main to States extending their tax charge, under the pressure of the need of revenue, to all persons and to all income and property within their jurisdiction. When, therefore, an individual or corporation resides in one place but has its income or property in another, it may well find itself paying taxes to both States. The appropriate remedy for this state of affairs has been the subject of dispute over many years. Some countries, such as the United States, and Great Britain (in the case of tax paid in the Dominions), have provided relief for their own citizens or residents unilaterally by giving them an allowance against their own tax for foreign or Dominion taxes paid. A country which follows this course takes upon its own shoulders the whole cost of the relief, nor in the absence of reciprocity can it be sure that the barrier to trade has been completely swept away. There is, therefore, much to be said for the method of providing by Treaty or Agreement which has the double advantage of sharing the burden of relieving the double taxation as equitably as possible and at the same time eliminating double taxation between the contracting parties altogether.

In this particular sphere of international co-operation the League of Nations has done much valuable work. The present publication contains three Model Conventions, drawn up by the Second Regional Tax Conference at Mexico in 1943, for the Prevention of Double Taxation of Income, for the Prevention of Double Taxation of Successions (Inheritance), and for the Prevention of Fiscal Evasion. Then Model Conventions are based on the work of the General Meeting of Government Experts on Double Taxation and Fiscal Evasion, organised by the League of Nations in 1928 and the subsequent meetings of the Fiscal Committee of the League. The Models are designed to facilitate negotiations between States for the conclusion of such Agreements and to serve as a basis of discussion of the problems involved. The Model Conventions for the Prevention of Double Taxation of Income and of Successions proceed broadly on the basis of allocating to the country where the income arises or the property is situate the primary right of taxation: each country retains, however, the right to tax its own residents subject to giving them an allowance against its own tax for any tax period in the other country. This is not, of course, the only method of relieving double taxation: whether it is the best one raises very wide issues. The third Model Convention on the Prevention of Fiscal Evasion may seem a strange bedfellow for the other two but perhaps we could hardly expect two contracting States to relieve their subjects of double taxation and then watch them use each other's territory as a base for tax avoidance.

The Model Conventions are accompanied by a detailed commentary explaining their provisions. This commentary is not only of value as an aid to understanding a subject of some complexity but it also traces the history and implications of the proposals.

The Spirit of Enterprise. By EDGAR M. QUEENY. Charles Scribner's Sons, New York. 1943. x, 267 pp.

This is a refreshing book. It is written by a business man who is remarkably free from current superstitions, including those current amongst business men. For the most part it is a defence of traditional American enterprise against its totalitarian enemies, but the author shows here and there that he understands that the Liberal State has a positive rôle to play. If there are many such business men in America there is yet hope for the market economy.

The most interesting part of the book is the discussion of the T.N.E.C. evidence and reports. Some of the T.N.E.C. conclusions have been extensively quoted and appealed to in this country, and it is a pity that there is not sufficient recognition of the fact that the T.N.E.C. was by no means a scientific body. The T.N.E.C. was probably often right, but it was also sometimes wrong and it is therefore dangerous to quote it as authoritative. Mr. Queeny does not give us a satisfactory analysis of the T.N.E.C. reports but his observations are always commonsensical and suggestive. What emerges, and what is important, is that the positive Liberal State should not aim at too subtle a plan for freedom. The referee should not be too quick with his whistle nor too ready to order players off the field. The rules of the game may well allow for a little hurly-burly. Economists like Professor Hutt, who are working out the rules of the game of free enterprise, deserve the highest praise. But they should realise that refinement has its price as well as simplicity, and that of the two simplicity costs the less.

A. A. SHENFIELD.

SHORTER NOTICES

The Use and Abuse of Statistics, with special Reference to Australian Economic and Social Statistics in Peace and War. By F. R. E. MAULDON. University of Western Australia Text Books Board. Crawley. 1944. 3s.

This booklet is a reprint of some broadcast talks on Statistics given in Australia. After a short introduction explaining index numbers, averages, vital indices and the like, the author discusses the different publications containing economic and social statistics in Australia and assesses the significance of some of the results obtained.

Nazi War Finance and Banking. By OTTO NATHAN. Occasional Paper No. 20. National Bureau of Economic Research. New York. April, 1944. iv, 97 pp. 50 cents.

In this pamphlet a painstaking account of German banking legislation and tax structure during the war is given. The earlier chapters are largely descriptive and historical, but in the last chapter an analysis of German national income and war finance is attempted. A good deal of the material covers the same ground as Dr. Singer's articles on the German war economy which have appeared in the *Economic Journal.*

Prices in a War Economy. Some Aspects of the Present Price Structure of the United States. By FREDERICK C. MILLS. Occasional Paper No. 12. National Bureau of Economic Research. New York. October, 1943. 102 pp.

The sub-title of this pamphlet admirably describes its scope. It deals in great detail with changes in the general and some of the partial price levels in the United States and brings together a good deal of the relevant material. An appendix contains detailed American price statistics for the period 1927—1943 given at monthly intervals.

British and American Plans for International Currency Stabilisation. By J. H. RIDDLE. Occasional Paper No. 16. National Bureau of Economic Research. New York. December, 1943. 42 pp. 35 cents.

As the title indicates this pamphlet examines the Keynes and White plans for International Currency Stabilisation. About half of the space is taken up by a description of these plans' provisions, the rest of the pamphlet being devoted to an analysis of their implications, with special reference to the position of the United States.

Food Rationing and Supply 1943–4. League of Nations. Economic, Financial and Transit Department. Geneva. 1944. London: George Allen & Unwin, Ltd. 101 pp. 4s. 6d.

This work forms a sequel to an earlier League publication on *War Time Rationing and Consumption.* The greater part of the present book is taken up with a description of the different rationing systems in force in the autumn of 1943, and an attempt to calculate the calorific and nutritional values of the different diets allowed. Although comparisons between diets as a whole are not always possible, there are some interesting data regarding the consumption of particular foodstuffs in different countries. The second chapter deals with national health as measured by birth, death and morbidity rates; but in view of the restrictions of space, and the lack of data, little can be done to correlate these rates with the diets described in the first part.

Railroad Travel and the State of Business. By THOR HULTGREN. Occasional Paper No. 19. National Bureau of Economic Research. New York. December, 1943. 35 pp. 35 cents.

In this pamphlet fluctuations in business activity are related to railway travelling. The results are much the same as one would expect: there is a positive association between travelling and business activity, though highway development and the increased use of the motor car have tended to make this association less clear in the United States than it otherwise would have been.

The Post-War Depression and The Way Out. By B. R. SHENOY. Kitabistan, Allahabad, India. 1944. 159 pp.

In this short booklet Professor Shenoy has summarised the prevalent explanations of the process and machinery of saving, the chief methods adopted of financing the recent war and the causes of post-war booms and depressions. Finally he summarises the various methods or combinations of methods whereby a post-war depression might be avoided. His exposition is clear and concise, although—as was hardly unavoidable in a short booklet—somewhat incomplete on the theoretical side. He concludes that the chief cause of post-war fluctuations is in the finance of war through loans and inflation, but that even if, for political and administrative reasons, loans and inflation have not been avoided, these disastrous effects can be counteracted by a drastic post-war capital levy, strict control over bank credits, and the continuation of wartime controls during the period of transition. This is a useful little volume which should do much to explain to the general reader what are the principal economic legacies of the recent war and why it is necessary to continue war-controls during the immediate post-war period.

The Bombay Plan: A Review of its Financial Provisions. By B. R. SHENOY. Karnatak Publishing House, Bombay. 1944. 44 pp. Rs. 1/4.

This pamphlet summarises and criticises the financial provisions of the now well-known " Bombay Plan ", and concludes that the authors of the Plan have committed errors of double-counting and have failed to realise the unavoidably inflationary effects of created money, with the result that they have estimated the capacity of the Indian economy to generate capital for reconstruction and industrialisation at about double its actual amount. The analysis of the effects of created money and of forced savings is clear and convincing, but Professor Stenoy has surely underrated the extent to which sterling assets might safely be used to finance investment in productive undertakings, and to which the introduction of more efficient methods of production can increase output and incomes. Hence although the authors of the Plan are undoubtedly overoptimistic, the extent of their exaggeration of India's capital capacity is less serious than Professor Shenoy would have us believe.

Zur Dynamik des Spätkapitalismus. By NATALIE MOSZKOWSKA. Verlag " Der Aufbruch ", Zürich/New York. 1943. 185 pp.

Dr. Moszkowska has set herself to investigate the major causes of the social, economic and political ills of our time. She approaches her task from the point of view of orthodox Marxism. Apparently she does not know the work of Lange and Lerner, and she evidently has never heard of Lord Keynes. Marginal utility theory is to her " une plaisanterie viennoise " (p. 16). Thus lightly equipped she struggles bravely with the problem of over-accumulation in a world in which savings and investment cannot diverge even *ex ante*. Her main thesis is that in declining Capitalism the rate of profit still rises owing to technical progress, but that the " faux frais " of the system (costs of advertising, armaments, wars, etc.) absorb ever larger proportions of the total surplus value, so that real wages cannot increase.

Occasionally, the drab landscape of undiluted Marxism is lit by a flash of genuine insight: she rightly points out that labour-saving innovations may diminish the total value of capital.

Studies in Business Administration, Vol. XIV, No. 4. (*Supplement to the Journal of Business of the University of Chicago.*) " Secular Trends and Idle Resources ". By M. V. JONES. University of Chicago Press. Cambridge University Press. 1944. iv, 72 pp. $1.00.

In this study, an examination of the secular stagnation theory mainly as developed by Professor Hansen, the author does not contest Hansen's basic savings-investment theory, but takes Professor Hansen on his own ground. After an introductory chapter summarising the theory he inquires successively whether technological, population and settlement trends lessen investment relatively to savings. His answer is no. Then he contests Hansen's argument that the twenties were only prosperous because the maturing of the American economy was offset by several special forces, such as the huge volume of building. He argues that most of the theory is either fallacious or quantitatively insignificant, but unfortunately makes no mention of the important argument that intensive expansion is stimulated by extensive expansion. This is only justifiable if his arguments regarding extensive expansion are correct. Furthermore, in some places he seems to misinterpret Hansen's arguments, probably owing to their excessive brevity. (Surely the importance of capital-saving innovations lies in their effect on investment, not on consumption.) Thus the study shows the need for an elaboration of Hansen's theory. It also points out that too little empirical verification has been attempted. It cannot be said to be the last word on the subject, however.

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